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DEPARTMENT OF ECONOMICS



## **ESSAYS IN LABOR AND PUBLIC ECONOMICS**

submitted by

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*Für meine Mutter, para mi Padre, et pour Anna-Maria*

*Facilis descensus Averni;  
Noctes atque dies patet atri janua Ditis;  
Sed revocare gradum, superasque evadere ad auras,  
Hoc opus, hic labor est.*

Virgil



## Declarations

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### Statement of Conjoint Work

I confirm that Chapter 1 was jointly co-authored with Martin Murmann and I contributed at least 50% of this work. I confirm that Chapter 2 was jointly co-authored with Michelle Hansch and I contributed at least 50% of this work. I confirm that Chapter 3 was single authored by myself and I contributed 100% of this work.

### Disclaimers

Chapter 1 of this thesis uses a new dataset we created by combining administrative employer-employee linked data from the Institute for Employment Research of the German Federal Employment Agency (*IAB*) with the *Start-Up Panel* provided by the *IAB* and the Leibniz Centre for European Economic Research (*ZEW Mannheim*).

Chapter 2 of this thesis uses administrative social insurance data from the Continuous Working Life Sample (*Muestra Continua de Vidas Laborales (MCVL)*) that is provided by the Spanish Social Insurance Authority (*Seguridad Social*).

Chapter 3 of this thesis uses the Germany-specific data of the *OECD's Program for International Student Assessment (PISA)* and is provided by the Institute for Educational Quality Improvement (*Institut zur Qualitätsentwicklung im Bildungswesen (IQB)*).

The views expressed in this dissertation are solely those of the author and do not necessarily reflect the views of the data providers. I am grateful to each of them for having enabled me to get access to the data as described in the data section of each chapter.

### Eidesstattliche Erklärung

Hiermit erkläre ich, die vorliegende Dissertation selbstständig angefertigt und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient zu haben. Insbesondere sind sämtliche Zitate aus anderen Quellen als solche gekennzeichnet und mit Quellenangaben versehen.

Mannheim, 09.03.2020

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Sebastian Camarero Garcia

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# ESSAYS IN LABOR AND PUBLIC ECONOMICS

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## ABSTRACT

This doctoral thesis contains three essays on labor and public economics with a focus on investigating the effects of labor market policies on entrepreneurship and public policies on inequality of opportunity. In particular, it analyzes the role of both policy variables of the unemployment insurance (UI), the potential UI benefit duration (i.e. how long an individual is eligible to receive benefits) and the level of UI benefits (i.e. how much support one gets during unemployment), on self-employment. Moreover, this dissertation investigates the role of learning intensity on Inequality of Educational Opportunity (IEOp). Thereby, the reader should note that the three chapters of this dissertation can be read independently.

**Chapter 1** is titled “Unemployment Benefit Duration and Startup Success” and coauthored with Martin Murmann. The first chapter analyzes how the potential benefit duration (PBD) of the unemployment insurance (UI) affects the actual unemployment duration of founders, their motivation for starting up and ultimately their subsequent performance, i.e. startup success. Despite the importance of business creation for the economy and even though a relevant share of new firms is started out of unemployment<sup>1</sup>, research has focused on analyzing the effect of UI policy on re-employment outcomes. To fill this gap, we create a novel representative dataset on founders in Germany that links established administrative social insurance data with representative survey data that are sampled from the universe of all startups in Germany. Exploiting reform-based and age-based exogenous variation in potential benefit duration (PBD) within the German UI system (in a time period when UI benefit levels as second policy variable remained fixed), this chapter derives causal estimates of PBD on three sets of outcomes. First, we find that longer PBD causally increases actual unemployment duration of those becoming self-employed. The UI duration elasticity for previously unemployed founders (0.6) is higher in comparison to common estimates of the UI duration elasticity for those becoming re-employed (0.15). Second, with increasing unemployment benefit duration, the founders’ outcomes in terms of self-assessed motivation change: the share of *necessity*- relative to *opportunity*-driven startups increases. Third, longer PBD also affects objective measures of startup success: sales and employment growth over the initial years after starting up become inferior.

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<sup>1</sup>In Germany about 25% of all startups every year, in Spain on which I focus in Chapter 2 even up to 50% of all startups are created out of unemployment every year.

**Chapter 1** develops an extended search model that shows how these net causal effects can be rationalized by a mix of *composition* and *individual-level duration* effects. The model illustrates that there may be policy conclusions to learn from the empirical results: for instance, targeted early retraining for unemployed individuals could help them to improve their potential to get re-employed instead of being pushed into self-employment at the end of their unemployment benefit spell. Moreover, this chapter suggests that the fiscal externality of UI on startup success should be considered for the (optimal) design of the UI system, because in the current literature this channel is mostly neglected and only the transition from unemployment to re-employment is taken into account.

**Chapter 2** is titled “Unemployment Benefits and the Transition into Self-Employment” and is coauthored with Michelle Hansch. The second chapter complements the first one by analyzing the role of the second policy variable of unemployment insurance (UI), the benefit levels, on self-employment. In other words, this chapter focuses on a setting in which the potential benefit duration (PBD) schedule is given in order to analyze the effect of UI benefit levels on the probability to exit from unemployment into self-employment or employment. To shed light on this issue, we use Spanish administrative social insurance data including so far inaccessible information on self-employment. Focusing on the Spanish UI system, we exploit reform-driven variation in UI benefit levels to identify the causal effect of UI benefits on total employment. Thereby, we cover the full picture by decomposing the overall effect into the causal effects on transitions from unemployment to employment, and self-employment. Documenting the evolution of all relevant labor market status flows in Spain over the business cycle (2005-2017), we consider two estimation strategies. First, we apply a Difference-in-Differences strategy which exploits a 2012 reform which led to a reduction in the level of UI benefits for those remaining unemployed for more than six months. Second, we propose a Regression Discontinuity Design which relies on the time interval between the UI entry date and the sharp reform cutoff date. We find the causal effect of reducing UI benefits on post-unemployment outcomes to differ: it increases employment (job-finding rate) but not self-employment (startup rate). Unemployed individuals adapt their job search behavior to avoid the drop in benefits after six months, whereas the decision for self-employment is less affected. We present novel insights that the UI benefit level duration elasticity is higher for employment than for self-employment. Our results suggest that the differential role of UI benefits on post-unemployment outcomes should be considered for the design of UI systems.

Taken together, **Chapter 1** and **2** contribute to the literature in labor and public economics by providing evidence on the role of both policy variables of the unemployment insurance (PBD and the level of UI benefits) on the so far neglected post-unemployment outcome, self-employment. The findings are based on administrative data from Germany and Spain, and thus policy conclusions are of interest at least for countries with similar European labor market institutions. The results should motivate empirical work for other countries and further theoretical work with respect to the potential welfare considerations (e.g. the optimal design of social insurance considering self-employment, etc.).

Chapter 3 is titled “Inequality of Educational Opportunities and the Role of Learning Intensity”. The third chapter turns to a topic from the realm of economics of education which can be considered to constitute a sub-field of both labor and public economics. This chapter investigates how increasing learning intensity affects inequality of educational opportunities, and thus indirectly social mobility. For this purpose, I exploit a German school reform that over the 2000s shortened the duration of secondary school by one year while maintaining the curriculum unchanged. Thus, this reform considerably increased the learning intensity because more curricular content had to be taught within a shorter total time period and lessons originally used for revision were now used to teach new material. Exploiting quasi-experimental variation due to the staggered introduction of this reform across the German federal states allows me to identify the causal effect of increased learning intensity (the ratio of curricular content covered per year) on Inequality of Educational Opportunity (IEOp). IEOp is estimated to be the share in educational outcome variance (as measured by the OECD PISA test scores) that can be explained by predetermined circumstances beyond a student’s control (e.g. the socioeconomic background).

The findings show that the reform-induced increase in learning intensity causally aggravated IEOp. This result appears to be driven by differences in parental resources that gained importance through support opportunities like private tuition adapting to the intensified educational process. Thus, differential compensation possibilities (depending on the parental background) appear to be the main mechanism for explaining how higher learning intensity causally increases IEOp. Moreover, results point to the existence of subject-dependent curricular flexibilities, with mathematics/science being more inflexible, that is, more responsive to changes in curricular intensity compared to reading. My findings suggest that it is important to account for distributional consequences when evaluating reforms aimed at increasing the efficiency of educational systems. My results also point to the relevance of taking into account the role of learning intensity when evaluating educational reforms. Therefore, Chapter 3 shows that learning intensity is an important aspect in human capital formation that can explain changes in educational opportunities, and thus ultimately impact social mobility. This should be considered when thinking about how best to design schooling to reconcile efficiency and equity considerations.

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**List of Abbreviations**

ABD	actual unemployment benefit duration
ATE	Average Treatment Effect
ATET	Average Treatment Effect on the Treated
DiD	Difference-in-Differences
EEOp	Equality of Educational Opportunity
EOp	Equality of Opportunity
EU	European Union
FTE	full-time equivalent
G-8 Model	Gymnasium-8 model
G-8 Reform	Gymnasium-8 reform
G-9 Model	Gymnasium-9 model
GDP	Gross Domestic Product
IAB	Institute for Employment Research of the Federal Employment Agency
IEOp	Inequality of Educational Opportunity
INE	Instituto Nacional de Estadística
IOp	Inequality of Opportunity
IQB	Institut zur Qualitätsentwicklung im Bildungswesen
ISCED	International Standard Classification of Education
ISCO	International Standard Classification of Occupation
ISEI	International Socio-Economic Index of Occupational Status
IV	Instrumental Variable
MCVL	Continuous Working Life Sample ( <i>Muestra Continua de Vidas Laborales</i> )
OECD	Organization of Economic Co-operation and Development
PBD	potential benefit duration
PISA	Program for International Student Assessment
RDD	Regression Discontinuity Design
SC	Standing Conference of the Ministers of Education and Cultural Affairs
SE	self-employment
SES	socio-economic status
UA	unemployment assistance
UE	unemployment
UI	unemployment insurance
ZEW	Leibniz Centre for European Economic Research

## Glossary

**Gymnasium** is the academic track of secondary school education in Germany covering both lower and upper secondary level (grades 5–13 or 5–12) and providing an in-depth general education aimed at the general higher education entrance qualification (*Allgemeine Hochschulreife*)

**Learning Intensity** is the ratio of curricular content covered in a given period of time. In particular, the G-8 reform led to increased learning intensity in such a way that by the end of grade 9 post-reform, students have received about the same amount of instruction, and covered the same curriculum as students that had completed two-thirds of grade 10 pre-reform. Learning intensity, thus, corresponds to the *intensive* margin as it reflects the amount of content (curriculum) to be studied in a constant amount of instruction time, whereas school duration (e.g. number of years/days) refers to the *extensive* margin

**Plausible Value** Following OECD (2009; PISA Data Analysis Manual) in chapter 6: Instead of directly estimating a student's ability  $\theta$ , a probability distribution is estimated. Thus, instead of obtaining a point estimate, a range of possible values with an associated probability for each is estimated. Plausible Values are random draws from this (estimated) distribution



## **Chapter 1**

# **Unemployment Benefit Duration and Startup Success**

## 1.1 Introduction

Business creation plays an important economic role in stimulating productivity, fostering structural change, and foremost, offering jobs for their founders and additional employees (Aghion et al. 2009, Dent et al. 2016, Haltiwanger et al. 2013). Self-employment accounts for 10-15 percent of the labor force in most OECD countries. Thereby, a little known fact is that each year, one quarter of all new business creations is started out of unemployment.<sup>1</sup> Due to the spread of new forms of employment in the digital economy (e.g. Uber drivers), the relevance of transitions from unemployment to self-employment can be expected to increase in the near future. Recent research in public economics demonstrates that the generosity of **unemployment insurance (UI)** systems in terms of **potential benefit duration (PBD)** and benefit levels affects re-employment outcomes of those transitioning from unemployment to employment. Despite their economic importance, however, transitions from unemployment to self-employment (business creations) are largely ignored and little is known about how the design of unemployment insurance affects the behavior of potential entrepreneurs and the subsequent performance of their firms. Chapter 1 therefore sheds light on this topic by analyzing how the potential **UI** benefit duration (**PBD**) affects the **actual unemployment benefit duration (ABD)** of those transitioning to self-employment, their motivation for starting a firm, and the success of their firms in terms of sales and employment growth.<sup>2</sup>

The effect of longer potential benefit duration (**PBD**) on self-employment outcomes seems a priori unclear. On the one hand, if longer **PBD** incentivizes longer actual unemployment, losses in financial, social, and human capital might lead to a gradual decrease in startup quality. For instance, longer unemployment duration could decrease financial means, increase difficulties to attract external financial capital (due to stigmatization), lead to losses of business contacts, or a depreciation of skills and knowledge.

On the other hand, a period of unemployment might be used to better prepare for self-employment, e.g. by acquiring new skills or developing market entry strategies. Apart from these *individual-level duration effects*, there are likely to be *composition effects*, as individuals with high motivation and ability possibly leave unemployment the fastest. Thus, with longer unemployment duration, a higher amount of individuals with low ability and motivation could remain to found a startup. In our study, we identify the overall causal effect of potential **UI** benefit duration on startup success and thus the (net) result of the government's **UI** policy. We rationalize which mechanisms could explain our results with the help of post-hoc analyses and a formalized job search model.

---

<sup>1</sup>In Germany, the empirical setting of this chapter, around 25-30 percent of all founders between 2005 to 2015 have been unemployed before starting their firms. According to data from the Mannheim Enterprise Panel and the ZEW/IAB Start-Up Panel, there were around 200,000 startups each year between 2000 and 2015. This corresponds to about 50,000-60,000 startups out of unemployment per year.

<sup>2</sup>Concerning the notation in this chapter: with *self-employment*, we refer to the labor market status to distinguish unemployment, employment and self-employment. Within the labor market status of self-employment, the term *founder* refers to the person starting a firm which covers both firms with and without employees. The term *entrepreneur* is used to focus on a founder who continues to run a firm after starting it. The term *startup* refers to the act of starting a firm and is used as a synonym for *new firm*.

In this chapter, we construct a dataset that links a representative panel survey of founders in Germany to administrative data on their own and their employees' labor market histories. Thereby, we obtain a representative linked employer-employee dataset for founders. Due to their high level of detail, German administrative data on labor market histories have been widely used in research (e.g. [von Wachter & Bender 2006](#), [Dustmann et al. 2009](#)), allowing for a good comparability of our measures with those used by previous research in the context of transitions to dependent employment. Our data enable us to consider two types of outcome variables, the founders' self-assessment as measured by their motivation to start up (*necessity/opportunity* driven entrepreneurship), and their objectively measurable outcomes, such as sales and employment growth during the first years of business. We focus on startup growth as outcome, since growth potential in the first years should be more directly influenced by unemployment duration than more distant outcomes, which ultimately allows a clear attribution of the measured effects. Early growth is also a predictor of long-term firm success ([Sedláček & Sterk 2017](#)).

To identify the causal effects of unemployment benefit duration on startup success, we exploit exogenous variation through both policy reforms of **PBD**, and age-specific cutoffs in the **PBD** schedule of the German unemployment insurance (**UI**). Our main empirical strategy is an **Instrumental Variable (IV)** approach. We instrument **PBD** (and actual unemployment benefit duration) with the interaction term “being in the relevant age cohort” (only those above 45 years were affected by reforms) and “becoming unemployed after the reform changed maximum **PBD**”. The reforms of 2006 and 2008 jointly reduced maximum **PBD** by at least six months for affected cohorts. Our **IV** approach entails the features of both **Difference-in-Differences (DiD)** and **Regression Discontinuity Design (RDD)**. In general, results remain robust when conducting a **RDD** to estimate the effect induced by age cutoffs in the **PBD** schedule or a **DiD** approach based on policy reforms.

Our results show a net negative causal effect of longer **PBD** on startup success. More specifically, our estimates suggest that longer **PBD** increases actual unemployment duration implying an **UI** elasticity of around 0.6, which is higher than what recent estimates of 0.15 suggest when focusing on dependent re-employment (see, e.g. [Schmieder & von Wachter 2016](#)). Via this channel, longer **PBD** significantly increases the likelihood that individuals start firms out of *necessity* - compared to a situation in which they start the firm because of an *opportunity* motive - by about two percent per additional month of **PBD**. Moreover, and particularly when focusing on the non-manufacturing sectors, we consistently find a negative effect of longer **PBD** on actual outcomes in terms of employment and sales growth in the first two years after starting up.

These net causal effects can be driven by *individual-level duration*, *composition effects*, or a mix of both. We explain this with a stylized search model in [Section 1.5](#). Empirically we find limited evidence for *composition effects* in observable characteristics of unemployed founders in response to **UI** policy changes.

Hence, our findings suggest that the net effect is mostly driven by an *individual-level duration effect*, i.e. that the ability of individuals to succeed as entrepreneurs depreciates the longer they are unemployed. For instance, access to credits may deteriorate with longer unemployment duration and thus financially constrain the respective startups' growth potential. The results therefore indicate that by setting the length of **PBD**, the government can affect the quality of firms starting out of unemployment. Through this channel, changes in **PBD** may induce considerable fiscal externalities (Lawson 2017) which affect the cost-benefit analysis of the **UI** system depending on the startup success (out of unemployment). This should also be considered when evaluating the value of insurance for self-employment (compare e.g. Hombert et al. 2020).

Our study makes three major contributions. First, we document how the **PBD** in the unemployment insurance affects the actual unemployment duration of future entrepreneurs. In this context, it is ex-ante unclear whether the positive causal effect of **PBD** on actual **UI** benefit duration – which is usually found for those becoming re-employed as wage workers – also exists for those starting a business (and whether we can expect similar effect sizes). This has direct implications for the cost of **UI** systems since additional fiscal externalities have to be considered because of the transitions from unemployment to self-employment.

Second, we provide evidence on how the **UI** policy affects the motivation of founders, i.e. whether they start a firm because of a business *opportunity* or out of *necessity* (as a last resort). In this way, our study illustrates how **UI** policies can serve as a tool to maximize the share of *opportunity*-driven startups. These types of startups might have the highest potential for generating long-term economic value. Our results further suggest on this score that self-classifications can serve as an important indicator for the future potential of a startup and are therefore worthy of attention in themselves.

Third, we facilitate an understanding of how the growth potential (success) of startups in terms of employment generation and sales depends on **UI** policy. This is important for decisions on the optimal design of unemployment insurances since it affects their cost-benefit ratio. There are also implications for the optimal design of active labor market policies which incentivize unemployed individuals to transition into self-employment, particularly regarding the question of when such policies should come into practice. Thus, our results are informative for nascent entrepreneurs, money lenders, and policy makers.

This chapter of my dissertation connects several strands of the literature on entrepreneurship and public economics which have so far evolved in parallel to each other. First, research on entrepreneurship has investigated potential determinants of becoming a firm founder (e.g. Evans & Leighton 1989b, Berglann et al. 2011, Levine & Rubinstein 2017). Stylized facts suggest that unemployment increases the propensity to become self-employed (e.g. Evans & Leighton 1989a, 1990, Meager 1992, Blanchflower & Meyer 1994, Kuhn & Schuetze 2001, von Greiff 2009, Røed & Skogstrøm 2014b,a) but that previously unemployed founders perform worse in entrepreneurship than those transitioning from dependent employment (e.g. Andersson & Wadensjö 2007).

However, the entrepreneurship literature so far has largely ignored significant heterogeneity among the unemployed in terms of their motivation to start a business, and their firms' subsequent performance. In addition, the issue of *necessity*- versus *opportunity*-driven entrepreneurship has only been discussed in the context of very specific active labor market policies (e.g. [Caliendo & Künn 2011](#), [Caliendo & Kritikos 2010](#), for the case of startup subsidies) but not in the more important context of the general **UI** system. In studying the effects of **PBD** on the timing of when unemployed individuals move (or are pushed) into self-employment (including its subsequent outcomes), we contribute to the entrepreneurship literature by providing evidence for the potential implications of the **UI** system on the success of firms started by unemployed individuals.<sup>3</sup>

Second, this project also adds to the literature of public economics on the optimal design of unemployment insurance (**UI**) by providing evidence for the effect of potential benefit duration (**PBD**) on future entrepreneurs. The public economics literature has discussed several aspects concerning the optimal design of **UI** policies, i.e. the level of benefits and their eligible duration (e.g. [Hopenhayn & Nicolini 1997](#), [Katz & Meyer 1990a,b](#), [Lalive 2008](#), [Schmieder et al. 2012, 2016](#), [Kolsrud et al. 2018](#)). Its focus has been on investigating effects on subsequent employment outcomes, those predominantly being re-employment wages (e.g. [Le Barbanchon 2016](#), [Schmieder et al. 2016](#), [Le Barbanchon et al. 2019](#), [Nekoei & Weber 2017](#)). Results suggest that increases in potential benefit duration (**PBD**) lead to increases in actual unemployment duration (**ABD**). However, the effects of longer actual unemployment on re-employment wages remain disputed. For instance, [Nekoei & Weber \(2017\)](#) argue that longer **PBD** can either induce a delay in job acceptance (and simply subsidize leisure) or be beneficial by improving job opportunities (through subsidizing a longer search that results in job matches of higher quality). While [Nekoei & Weber \(2017\)](#) find that the latter positive effect dominates in Austria, [Schmieder et al. \(2016\)](#) report negative effects of unemployment duration on re-employment wages in Germany. We contribute to this debate by providing evidence of the causal effect of unemployment benefit duration on self-employment outcomes based on our newly created dataset. Thus, the first chapter of my dissertation ultimately complements the analysis of **UI** benefits with respect to post-unemployment outcomes ([Jarosch & Pilossoph 2019](#)).

We proceed as follows: in [Section 1.2](#), we explain our data construction and conduct a descriptive analysis. [Section 1.3](#) illustrates the institutional background and our identification strategies for deriving causal effects. In [Section 1.4](#), we present our empirical estimates which we rationalize in a stylized model in [Section 1.5](#). [Section 1.6](#) provides our conclusion.

---

<sup>3</sup>Though self-employment is a smaller fraction of the total labor force (e.g. 10% in Germany) than employment, founders can be considered incubators of (re-)employment. Even if only five percent of all the unemployed start a firm, the employment spillovers are significant: if a startup employs on average two employees after one year (three after three years), this means that we talk de facto about at least 15 (to 20) percent of the unemployment stock that may benefit from those startups, which neglects the fact that through new startups individuals may be saved from becoming unemployed in the first place.

## 1.2 Data and Descriptive Analysis

### 1.2.1 Dataset

For this chapter’s empirical analysis, we constructed data that matches the employer information of the IAB/ZEW Start-Up Panel with employee register data from the statistics of the German Federal Employment Agency. In this way, we circumvent the data limitation that German employer-employee linked administrative social security data normally put on any information regarding self-employed individuals. Self-employed individuals in Germany are not obliged to contribute to the public social security system. In contrast, it is mandatory for regular dependent employees to make social security contributions.

The IAB/ZEW Start-Up Panel is a joint research project from the [Institute for Employment Research of the Federal Employment Agency \(IAB\)](#), the [Leibniz Centre for European Economic Research \(ZEW\)](#), and Creditreform, Germany’s largest credit rating agency (see [Fryges et al. 2010](#), for details on the sample design of the dataset). This dataset is a sample taken from the Mannheim Enterprise Panel (“MUP”) which contains basic information on almost the entire universe of firms in Germany, including start-ups. The information is collected by Creditreform, which conducts credit ratings for basically all firms in Germany ([Bersch et al. 2014](#)). To be more precise, the Start-Up Panel is a random sample of the MUP providing a representative dataset of young firms from almost all industries (the primary sector, public sector and energy sector are excluded). Information is collected by means of a yearly telephone survey (computer-aided telephone interviews, CATI). The sample of the [IAB/ZEW](#) Start-Up Panel is stratified by the year of firm formation and by industry sector. Stratification is controlled for by including dummy variables for the stratification cells in all regressions. Currently, the [IAB/ZEW](#) Start-Up Panel contains data on more than 21,000 firms founded between 2005 and 2015.<sup>4</sup>

The linked register data are drawn from the “Integrated Employment Biographies” provided by the German Federal Employment Agency. These administrative data yield information on the start and end dates of all employment and unemployment spells in the founders’ (and the start-ups’ employees’) employment histories, and on their potential unemployment benefit duration. The data are reported by the social insurance agencies. They are generated by the employing establishment and collected by the employment agency. Due to their high level of detail, the data are widely used in scientific research (e.g. [von Wachter & Bender 2006](#), [Dustmann et al. 2009](#), [Schmieder et al. 2016](#)).

We matched the founders’ and the start-ups’ employees employment histories from the German Federal Employment Agency with the firm-level data of the [IAB/ZEW](#) Start-Up Panel by applying text search algorithm methods. Thus, we obtained the most representative employer-employee linked dataset for founders in Germany as of this date.

---

<sup>4</sup>The first survey wave was conducted in 2008, collecting data on firms founded between 2005 to 2007.



Our matched dataset covers longitudinal information on approximately 18,000 start-ups.<sup>5</sup> Comparing survey with recorded administrative data, our dataset already reveals some interesting observations. In the **IAB/ZEW** Start-Up Panel survey, about 15% of interviewees state that at least one founder in the team has been unemployed just before starting up the new venture. Our linked dataset confirms that almost 76% of founders are classified identically in the survey and administrative register data when it comes to the founder's previous labor market history. Only 20% of founders who have some registered unemployment spell before becoming self-employed do not reveal that they have been unemployed in the survey. Instead, less than four percent of founders report to have started a firm out of unemployment when there is, in fact, no unemployment-related entry in their social insurance records. In summary, these patterns are in line with differences between survey and administrative data concerning individual labor market histories that have been observed in the psychology literature. In fact, feeling ashamed of not having a job may lead to under-reporting of unemployment (e.g. [Chletsos et al. 2013](#)).

The high percentage of identical classifications and further quality checks conducted on the matching process confirm the quality of our survey data on founders. Moreover, the conducted robustness checks also confirm the quality of our new dataset which links the survey data with administrative data (Appendix **I.2**). In conclusion, the created linked dataset allows us to derive representative results for founders in Germany and appears to be the most appropriate resource for this purpose that is currently available.

### 1.2.2 Descriptive Analysis

For the aim of this study, we focus on the approximately 12,000 cases in which the firm was started by a single founder (as opposed to a team). The reason for this decision is that in order to detect the effect of **PBD** on subsequent firm outcomes, it is important to identify the unemployment duration effect, which is most clearly possible analyzing a single founder, i.e. a non-team founder (as opposed to a team of founders).

Moreover, our dataset includes both non-team founders that have and those that do not have any employees. In the main empirical analysis, we focus on roughly 1,300 firms whose non-team-founders were unemployed directly before starting their firms. They were between 35 and 65 years of age when entering unemployment, and became unemployed before some major reforms led to changes in the availability of start-up subsidies for the unemployed in 2012. We only include individuals for which all required information on control variables is available and who have collected enough contribution months to be entitled to receive benefits for the maximum **PBD**.

---

<sup>5</sup>We were able to match labor market histories of about 80% of the founders from the **IAB/ZEW** Start-up Panel based on their names, birth date, and additional geographical information. Given that not all founders were necessarily employed i.e. subject to social insurance in Germany before (e.g. as they have always been self-employed), this is a very high ratio of matched individuals. Moreover, we were able to match establishment data to about 90% of those start-ups that self-reported employees subject to registration with the German social insurance based on the establishments' names and addresses. Self-reported information is taken from an interview. For more details on the construction of our dataset, see Appendix **I.2**.

Detailed summary statistics for all variables are shown in [Table 1.2](#): i.e. for our regression sample of previously unemployed founders (and those of them with above median unemployment duration) as well as for a reference group of previously employed founders. The non-team founders are typically male (85%) and of German origin (94%). They have on average 17 years of experience in the industry in which they start a firm, and most of them (85%) have never been self-employed before entering unemployment. The founders are on average 44.44 years old, about 39% enter unemployment when they are at least 45 years old (and hence belong to the treatment group in the subsequent causal analysis). In terms of education, 28% of founders achieved university degrees and 13% of them held managerial positions in the 5 years before starting up. Having a [PBD](#) of 12.32 months, on average, the mean actual unemployment duration is 4.79 months before they enter self-employment.

[Figure 1.1](#) shows the outcomes in terms of employment and sales per year for all entrepreneurs in the linked dataset. [Figure 1.2](#) compares outcomes of all entrepreneurs in our sample, distinguishing those who started their business out of unemployment with those that became entrepreneurs out of regular employment (marked as not unemployed in [Figure 1.2](#))<sup>6</sup>. The results show large differences between the two groups in terms of [full-time equivalent \(FTE\)](#) employment and sales. Having been unemployed before starting a firm is associated with inferior outcomes when becoming an entrepreneur, both in terms of the levels but also the growth trajectory of subsequent firm outcomes. Those who survive seven years as self-employed ([self-employment \(SE\)](#)), entering [SE](#) out of employment, can on average increase their sales by more than 100,000 Euro per year. Instead, founders who were previously unemployed and survive seven years, on average, only experience concave-shaped sales growth: they achieve an average increase in sales per year from about 150,000 to 300,000 Euro after seven years. Similarly, they only manage to increase [FTE](#) employment from about 0.5 to 1.5 employees (compared to an increase from 1 to 2 [FTE](#) employees in the other group).

Zooming in on those entering self-employment out of unemployment, [Figure 1.3](#) compares outcomes of the previously unemployed entrepreneurs in our sample, split at the median unemployment duration. The results show that large differences between the two groups evolve over time in terms of both [FTE](#) employment and sales per year. Longer prior unemployment duration before starting a firm seems therefore to be associated with inferior outcomes as an entrepreneur. This lower growth potential is not necessarily visible in the year of foundation but develops over the first years of a firm's existence. Our descriptive results indicate that a large part of the outcome differentials between previously unemployed and not unemployed founders are driven by unemployed founders with high unemployment duration.

---

<sup>6</sup>As explained in [Section 1.2.1](#) and in the figures' notes, we focus on non-team founders that are 35 to 65 years old. All firms are included independent of the survival length. Note, that all figures look very similar when we condition on firms that survive at least three or five years. The respective figures conditioning on survival are available upon request. They confirm that startup success is relevant (and not biased by survival).



One interesting feature from our dataset is the fact that we have information on the motivation of founders for starting up. Founders are asked about their motivation to start a business only during the first interview of the year in which they enter the panel survey (to best reflect the initial startup reason) and can only choose one answer category. [Table 1.1](#) shows how we define the different answer categories into either *necessity* driven motivation for starting up or into *opportunity* driven motivation for becoming an entrepreneur. In fact, we classify the answer categories “self-determined working” and the “realisation of business idea” as well as “better earning potential”<sup>7</sup> as indicators for founders that could be defined as being *opportunity* driven entrepreneurs. Instead, individuals answering “no suitable employment options” or “escape from unemployment” as main motives for starting a firm can be defined as belonging to the category of *necessity* driven or *pushed* entrepreneurs.<sup>8</sup>

[Figure 1.4](#) compares outcomes of all non-team founders in our sample, split by their self-reported motivation: being an *opportunity* or a *necessity/pushed* driven founder. The graphs reveal that those founders reporting an *opportunity*-driven motivation for starting their business experience faster growth in sales and full-time equivalent (FTE). This suggests that the motivation appears to be a good predictor for subsequent start-up success and that the notion of different types of entrepreneurs defined along the lines of their motivation describes observed differences adequately.

Finally, [Figure 1.5](#) compares outcomes of the previously unemployed founders in our sample, split by their self-reported motivation. It reveals considerable differences between the two groups in terms of FTE employment and sales. Starting a business out of unemployment with a *necessity* motivation is associated with worse outcomes over the self-employment spell compared to those launching a firm (out of unemployment) being motivated by *opportunity* considerations.

Conducting OLS regressions of the actual benefit duration on being classified as *necessity (pushed)* entrepreneur, sales and FTE employment (one and two years after having started the business) shows that the graphically observed correlations are quite robust. The relationships revealed in the descriptive analysis (as discussed in the five figures above) remain significant in the OLS regressions even after controlling for individual labor market experience, education, gender, nationality, and year as well as industry fixed effects ([Table 1.3](#)). In fact, simple regression analysis suggests that one month more of actual unemployment duration is associated with a 1.7 percentage point increase in *pushed* founders (five percentage in relative terms given an original basis of about 35 % *necessity* entrepreneurs). Moreover, one month of actual benefit duration is significantly correlated with a decrease in sales and FTE employment ([Table 1.3](#)).

<sup>7</sup>We tested all results by excluding founders who self-reported this motivation category. However, all results remain robust and thus we decided to maintain the classification as indicated in the main text.

<sup>8</sup>[Figure I-1](#) in [Appendix I.1](#) shows that *necessity* driven founders start up more often just before UI benefits expire, and thus appear to be *pushed* into self-employment. [Table 1.2](#) shows that one third of previously unemployed non-team founders indicate to have been *pushed* into self-employment.

These OLS results are reconfirmed in [Table 1.4](#) when focusing on startups in the non-manufacturing sectors (75% of our sample) . Non-manufacturing sectors offer easier market entry possibilities due to lower initial investment requirements and, thus, are most relevant for the entry of founders transitioning from unemployment to self-employment. In other words, [Tables 1.3](#) and [1.4](#) show that there is a statistically significant relationship between actual unemployment duration and subsequent self-employment outcomes.

In conclusion, the descriptive analysis already reveals that there appear to be significant differences for firm outcomes depending on the labor market history of the founder. In particular, starting a business out of unemployment is associated with worse outcomes in terms of sales and employment when compared to startups from founders who have been previously employed (not unemployed). Moreover, given previous unemployment history, longer unemployment duration seems to be correlated with worse self-assessment (more *necessity* in contrast to *opportunity* driven motivation for starting up) and subsequently worse firm outcomes.

### 1.3 Institutions and Empirical Strategy

The goal of this first chapter in my doctoral thesis is to find out whether potential benefit duration ([PBD](#)) causally affects the actual unemployment duration for the founders that start up out of unemployment and whether in consequence actual unemployment duration causally affects the motivation for starting a business as well as the subsequent firm outcomes. The main identification challenges lie in the fact that we need to exploit exogenous variation in [PBD](#) to learn how the length of eligible benefit duration causally affects actual unemployment duration ([ABD](#)), and hence how in general [ABD](#) affects outcome variables of interest. Otherwise, we face endogeneity problems. In theory, there may be, for instance, strategic behavior in becoming unemployed under the better [PBD](#) scheme conditions. Moreover, [PBD](#) (or actual unemployment duration) may be correlated with characteristics of unemployed people (e.g. previous working experience) that, in fact, explain the observed outcome. To solve these identification issues, we exploit policy reform and age-cutoff based exogenous variation in the [PBD](#) schedule within the German [UI](#) system.

We conduct an instrumental variables ([IV](#)) approach as main estimation strategy and check the robustness of our results by further conducting a regression discontinuity design ([RDD](#)) and a difference-in-difference ([DiD](#)) approach. This allows us to derive the net causal effect of [PBD](#) on the actual [UI](#) duration elasticity of founders, the motivation to become self-employed, and on objective measures of startup success. To begin with, we explain the main institutional features the identification strategies rely upon.

### 1.3.1 Institutional Background: German UI System and Reforms

In general, individuals in Germany who lose a job without fault of their own are entitled to unemployment insurance (UI) benefits (“Arbeitslosengeld I”) if they satisfy certain *eligibility constraints*. These require UI benefit claimants to have paid social insurance contributions for at least 12 months within the last two years (3 years before February 2006). The replacement rate has not changed since 1995 and is fixed at 60 percent of previous after-tax (net) earnings (67 percent if a person has dependent children). After exhausting UI benefits, one can get social security benefits tied to the existential minimum (“Arbeitslosengeld II”) which is subject to annual means testing.<sup>9</sup>

The potential benefit duration (PBD) depends, first, on an individual’s age at the start of the unemployment spell, and second, on the number of months worked in jobs covered by social insurance (*contribution months*) within a defined time period before claiming UI benefits (*coverage constraint*: 7 years before February 2006 and 5 years afterwards). For all workers satisfying the *eligibility constraints*, the PBD is 6 months, which corresponds to the 12 months of contributions paid before the UI spell starts (Table 1.5). Then, for each four additional *contribution months* before starting an UI spell, the PBD increases by two months. However, workers younger than 45 years can only reach a maximum PBD of 12 months, which corresponds to 24 months of contributions, i.e. they can not get more than 12 months PBD if they have collected more than 24 *contribution months*. This maximum PBD cutoff increases with the age. For instance, before February 2006, 30 months of contribution led to 15 months of PBD for workers equal or older than 45 years at the start of their UI spell. As Table 1.5 illustrates, workers older or equal to 57 years could reach with 64 months of contributions the maximum PBD of 32 months. Thus, they could acquire 20 months more PBD compared to a worker younger than 45 years who had also contributed 64 months just before entering UI in the same month.

While the PBD rules have been stable for workers that enter UI at an age younger than 45 years, the maximum PBD cutoffs have changed for the age groups over 45 years in February 2006 and a second time in January 2008. Each reform affected those individuals entering UI in the months after its implementation, whereas already unemployed individuals were still treated according to the rules in place in the month when they entered UI. Table 1.6 summarizes the eligibility criteria and changes over the different reforms<sup>10</sup>. The reform of 2006 led to a considerable reduction in the maximum PBD for all age groups above 45 years. The reform of 2008 led to a comparatively small increase in maximum PBD for some age groups above 50 years. In total, the net reform effect comparing the time period before February 2006 to that one after January 2008 can be characterized by a reduction in PBD for all age cohorts entering the UI system at an age older or equal to 45. The net effect is a reduction of at least six months (Table 1.6).

<sup>9</sup>In line with our data, the analysis focuses on 2005-2015. Thus, we describe the German UI system as it exists since 2005. Appendix I.3.1 gives more details on the labor market reforms in the early 2000s.

<sup>10</sup>For an overview of reforms in the German UI benefit before the time period studied in this chapter, see Schmieder et al. (2012). For the time period studied we refer to e.g. Price (2019) and Appendix I.3.1.

### 1.3.2 Main Empirical Strategy: Instrumental Variables (IV)

The identifying variation that we exploit in our three empirical estimation models stems from the age-dependent discontinuities in potential benefit duration (**PBD**) (Table 1.5) and from two reforms of the maximum **PBD** in 2006 and 2008 (Table 1.6).<sup>11</sup>

Our main empirical estimation models follow the instrumental variable (**IV**) approach of Le Barbanchon et al. (2019). The idea is to exploit the fact that the **PBD** in the German **UI** system depends on age-cut offs and that there have been reforms that only changed the **PBD** but not **UI** benefit levels. Thus, instrumenting **PBD** (or actual unemployment benefit duration) by an interaction of the reform and the age-cutoff is a useful instrument. It should satisfy the exclusion restriction because the differences in outcomes among individuals are unlikely to be explained by just small differentials in age (under or over the age cutoff) and the time when becoming unemployed (before or after the reform).

We estimate **IV** models of the form:

$$y_{it} = \alpha + \beta * Treated_{it} + \gamma * PBD_{it} + \delta * X_{it} + year_t + \varepsilon_{it} \quad (1.1)$$

$$y_{it} = \alpha + \beta * Treated_{it} + \gamma * ABD_{it} + \delta * X_{it} + year_t + \varepsilon_{it} \quad (1.2)$$

where for each founder  $i$  in month  $t$ :  $y$  is the outcome variable which can be motivation for starting a firm, sales or employment growth in the first and second year after foundation (i.e. yearly sales in Euro and yearly number of full-time equivalent (**FTE**) employees, both variables measured in logarithmic terms). Moreover,  $\alpha$  is a constant and  $X$  is a vector of firm- and founder-specific control variables (education, managerial experience, self-employment experience, industry experience, gender, being subsidized, industry-fixed effects). Finally, we control for macroeconomic conditions and trends in the unemployment or self-employment rate by taking into account year-fixed effects.<sup>12</sup>

The potential benefit duration ( $PBD_{it}$ ) and the actual benefit duration ( $ABD_{it}$ ) are instrumented by the instrumental variables:

- $IV06 = After(02/2006) * Treated(age \geq 45)$  which reflects the effect of a decrease in **PBD** by at least 6 months and/or
- $IV08 = After(01/2008) * Treated(50 \leq age \leq 54)$  which reflects the effect of an increase in **PBD** by at least 3 months.

<sup>11</sup>Startup Subsidies do not depend on age and though there was a change in the scheme of startup subsidies in 2006, first, we also use the 2008 reform as source of variation when relying on reform-based variation in **PBD**, and second, the age discontinuities we exploit exist at each point in time. Thus, our source of variation is not correlated with any changes occurring for the startup subsidies for the unemployed (cf. Appendix I.3.2). Moreover, we control in all regressions for the KfW-funding variable which is a proxy for most other forms of startup subsidies in Germany. Note that the purpose of this first chapter of my dissertation is to understand the role of the general **UI PBD** framework on the unemployed that exit into self-employment and not on rare active labor market policies. However, as most of these subsidies can be interpreted as an extension of **PBD**, learning about the general **PBD** effect on those who start a firm out-of-unemployment is important.

<sup>12</sup>We tested taking out observations from January 2006 so that the year effects fully capture the after-reform dummy. Conducting this approach does not alter our results.

This leads to the instrumental variable (IV) first-stage models:

$$PBD_{it} = \alpha + \beta * Treated_{it} + \gamma * IV06 (+\gamma * IV08) + \delta * X_{it} + year_t + \varepsilon_{it} \quad (1.3)$$

$$ABD_{it} = \alpha + \beta * Treated_{it} + \gamma * IV06 (+\gamma * IV08) + \delta * X_{it} + year_t + \varepsilon_{it} \quad (1.4)$$

The first-stage models may be regarded as tests about the strength of our instrumental variable (IV). As the instrumental variable should be correlated with the variable of interest, PBD (ABD), the F-Statistic of Equation (1.1) (Equation (1.2)) should be larger than 10 in order to avoid weak IV issues. In fact, our instruments turn out to be very strong, with Equation (1.1) yielding high F-statistics with values above 100 and always at least 10 in any specification (compare Table 1.9 to Table 1.16). In other words, the first-stage model (Equation (1.3)) proves that our instrument is a strong predictor of the instrumented variable of interest ( $PBD_{it}$ ). Moreover, one would expect that the corresponding F-statistic of Equation (1.2) will be smaller because the IV should be correlated in the first place with the policy variable that changed through the reforms, PBD, and only in second order with the actual benefit duration (ABD). However, we also instrument the ABD in order to understand how changes of PBD may affect subsequent outcomes of unemployed individuals that transfer from unemployment to self-employment and that are induced by changes in ABD initiated through the original change in PBD. Thereby, our IV estimator has the interpretation of a (local) average treatment effect of PBD/ABD on our outcomes - which is similar to the IV approach of Schmieder et al. (2016) that is used for estimating the wage effect.

Our IV approach exploits both reform-based and age cutoffs-based exogenous variation in order to estimate the causal effect of PBD on ABD, the motivation for starting a business, and on startup success. For robustness checks, we also conduct a regression discontinuity design (RDD) estimation that only exploits the age-cutoffs in the PBD schedule to derive the causal local average treatment effect (see Section 1.4.2). Finally, a difference-in-difference (DiD) strategy that only relies on the reforms in the PBD schedule allows us to estimate the causal treatment effect (see Section 1.4.2). Thus, our IV estimation approach has more external validity as compared to the two other estimation approaches because our IV strategy exploits the underlying exogenous variation in the explanatory variable used by both other strategies and thus entails them.

## 1.4 Results

In this section, we focus on the net causal effect of potential benefit duration (**PBD**) on actual unemployment benefit duration (**ABD**) and on the net causal effect of **PBD/ABD** on the motivation for starting up (out of unemployment) and on subsequent firm outcomes.

### 1.4.1 Main Results: Instrumental Variables (IV)

First, we estimate instrumental variable models as explained in the methods [Section 1.3](#). [Tables 1.9](#) and [1.10](#) as well as [Tables 1.11](#) and [1.12](#) show our main results that use the policy reform of 2006 as an instrument for longer potential benefit duration (**PBD**). [Tables 1.7](#) and [1.8](#) show our OLS baseline results. [Tables 1.13](#) and [1.14](#) as well as [Tables 1.15](#) and [1.16](#) show results of using both instrumental variables for the reforms of 2006, and 2008 (cf. [Section 1.3.2](#)). Note that we repeat all main results focusing only on non-manufacturing sectors. The reason is that the non-manufacturing sectors might be of particular relevance for market entry by unemployed individuals because entry into these sectors usually requires comparably low initial investment. Hence, focusing on non-manufacturing sectors may allow us to abstract from investment-driven unobserved heterogeneity. Importantly, all our results remain consistent and robust. Most findings are even more precisely estimated in the restricted sample (still 75% of our sample).

To begin with, for the case where we only conduct simple OLS regression with potential benefit duration (**PBD**) as the main explanatory variable, results on actual benefit duration (**ABD**), the motivation for starting a business and subsequent firm outcomes are shown in [Table 1.7](#). In all regressions, we control for education, previous labor market experience, individual characteristics (gender, nationality), industry, and year-fixed effects. More highly educated individuals tend to be less likely to start a business out of self-reported *necessity*. Previous managerial experience contributes to better performance in terms of sales and employment growth. Being female or foreigner does not have any differential effect concerning actual unemployment duration or the motivation for starting a business. If at all, these two characteristics may be associated with lower sales growth. Moreover, a funding dummy for subsidies from the Federal Employment Agency and from the KfW control for any potential concerns related to startup subsidies ([Appendix 1.3.2](#)).

The OLS results translate into a duration elasticity of about 0.5, as a one month increase in **PBD** increases **ABD** by 0.47 months. Moreover, one month of additional **PBD** increases the probability of starting a business out of necessity by about two percentage points. Finally, more **PBD** appears to imply less sales and full-time equivalent (**FTE**) employment growth in the first two years after starting the business. The results are confirmed and are measured more precisely when focusing on the non-manufacturing sectors (see [Table 1.8](#)).



However, to establish causality, we apply the instrumental variables (IV) approach as explained in the previous methods (Section 1.3.2). The causal IV estimates suggest that longer potential benefit duration (PBD) leads to longer actual unemployment benefit duration (ABD) before individuals transfer from unemployment to self-employment (see Table 1.9). This has been shown so far only with respect to re-employment. Via this channel, longer PBD increases the probability that individuals start a company out of *necessity* reasons (and not because they perceive a business *opportunity*). Moreover, longer PBD decreases firm performance in terms of employment and sales over the first years in business. The F-statistic is well above 10 in all versions of the IV models and hence indicates a good predictive power of the instrument (cf. Table 1.9). A one month increase in the PBD leads to a 0.6 month increase in the actual unemployment duration (column two of Table 1.9). In the second stage, this increase in ABD leads to a 1.5 percent higher probability to start a firm out of *necessity*. Given that the average probability to start a firm out of *necessity* is around 35 percent, this corresponds to a relative increase of five percent, which is economically significant (column three of Table 1.9). Turning to the effects on more objective outcomes, we find that only the effect on sales after two years remains of statistical significance. A one month increase in the PBD leads to 7.2% lower sales in the second year. Focusing on the non-manufacturing sectors, Table 1.10 shows that the negative effects of about one percentage point on FTE employment are statistically significant after the second year. Moreover, in the latter sample, the other effects get even stronger. In summary, our IV results confirm the initial OLS results.

Finally, our results are reconfirmed when using both IVs, that is IV06 for the 2006 reform, which decreased PBD by at least six months, and IV08 for the 2008 reform that increased PBD by three months. In fact, the results for changes in PBD on our outcomes of interest in Table 1.13 are very similar to those in Table 1.9 which are only based on IV06. The same is true when we focus on the non-manufacturing sectors. Table 1.14 based on two instrumental variables shows very similar results to the ones based only on IV06 (Table 1.10). We repeat the IV estimation instrumenting actual unemployment duration (ABD). The findings are shown in Tables 1.11 and 1.15 for IV06 as well as for IV06 and IV08 combined. They are in line with the described results for potential benefit duration. Similarly, Tables 1.12 and 1.16 show IV regressions based on actual benefit duration focusing on the non-manufacturing sector, and also confirm our main results.

## 1.4.2 Robustness Checks: Two Further Estimation Strategies

### Regression Discontinuity Design (RDD)

The RDD exploits age-dependent discontinuities in the potential benefit duration (PBD) estimating the equation:

$$y_i = \alpha \mathbf{1}(age_i \geq c) + f(age_i, \beta) + f(age_i, \gamma) \times \mathbf{1}(age_i \geq c) + X_i' \delta + \varepsilon_i \quad (1.5)$$

where  $y_i$  is the outcome variable of interest, i.e. actual unemployment duration and subsequent performance as an entrepreneur.  $X_i$  is a set of individual-specific covariates, and  $\varepsilon$  an error term. The dummy variable,  $\mathbf{1}(age_i \geq c)$ , reveals whether individuals benefit from extended PBD because they become unemployed at an age above the age-cutoff associated with higher maximum PBD. Individual age is the forcing variable for which we control with the  $\beta$  coefficient reflecting its direct effect on  $y_i$  and with the  $\gamma$  term reflecting its indirect effect on  $y_i$  via the interaction with the sharp age-cutoffs. Consequently,  $\alpha$  measures the pure discontinuity effect, i.e. the RDD estimate of interest, namely how PBD affects the outcomes of those who start a firm out of unemployment.

In the RDD, we focus on the period from 2008 onward, an age range from 45 until 54 years, and an age-cutoff at 50 years. Below an age of 50 years, the maximum PBD is 12 months. Above an age of 50 years, the maximum PBD is 15 months. This allows us to exploit an exogenous increase in the PBD of three months. Thereby, the identification assumption requires that there is no precise manipulation of the running variable around the cutoff (workers do not plan to become strategically unemployed just after an age threshold is reached to exploit higher PBD).<sup>13</sup> By restricting the regression sample to individuals who only become unemployed after January 2008, we see that they all face the same maximum PBD schedule as shown in columns (6) and (7) in Table 1.5 or 1.6. As in the time period studied no other major labor market reforms occurred and startup subsidies remained unchanged (cf. Appendix I.3.2), the only relevant difference in the PBD is driven by age-cutoffs. This can be tested by conducting a McCrary density test investigating whether there is bunching around the age-cutoff in the unemployment rate of workers becoming unemployed when aged between 45 and 54 years. Figure I-2 shows that the Mc-Crary test is passed, which is evidence suggesting that the identification assumptions of our RDD strategy are satisfied.

### Regression Discontinuity Design (RDD) Results

This regression discontinuity design (RDD) approach has been already exploited to study earlier reforms of maximum potential benefit duration (PBD) in the German UI system by Schmieder et al. (2012, 2016). These authors show that the RDD approach is credible for the time period studied (1987-2004). We take this empirical approach to investigate a more recent period (2003-2011). Moreover, we exploit the reformed age-dependent maximum PBD schedules with respect to a sample of individuals that is excluded in existing studies due to the data limitations that this dissertation chapter surpasses, i.e. we focus on those who transfer from unemployment to self-employment (not employment).

Results of the RDD are summarized in Figure I-2, I-3, I-4 and Table I.1. First, results of a McCrary density test do not indicate significant discontinuity in the distribution of individuals entering unemployment around the cutoff (Figure I-2).

<sup>13</sup>Le Barbanchon (2016) shows that this RDD approach works in the context of analyzing a French reform that increased PBD for certain unemployed individuals. Nekoei & Weber (2017) conduct a similar RDD estimating the effect of UI generosity on unemployment duration and re-employment wages in Austria.



Second, [Figure I-3](#) shows that our data construction process has been successful, because in line with the age-dependent rules on the maximal [PBD](#), as explained in [Table 1.5](#) and [Table 1.6](#), in the years since 2008, one would expect there to be an increase of around three months at the age of 50 for individuals who have contributed at least 30 months to social security. Indeed, our sample shows that the [PBD](#) increases, even though only by around 2.1 instead of 3 months (first panel in [Figure I-3](#)). Furthermore, our [RDD](#) strategy reveals that an increase of 2.1 months in potential unemployment benefit duration translates into a significant increase in actual unemployment duration of about 1.2 months. Thus, our [RDD](#) estimation approach suggests that the duration elasticity for unemployed persons that eventually start a business is around 0.6 (second panel in [Figure I-3](#)).

Unfortunately, the number of observations that can be used for the [RDD](#) is limited. Since information on sales is missing for a significant number of observations and sales are more volatile compared to employment outcomes, we are only able to derive meaningful [RDD](#) results for employment after one year and after two years ([Figure I-4](#)). The [RDD](#) results suggest that an increase of about 2.1 months decreases full-time equivalent ([FTE](#)) employment by about 12 percentage points in the first year and about 25 percentage points in the second year. This means that an increase of one month in [PBD](#) leads to a reduction in employment by about 6 percentage points in the first year and 12.5 percentage points in the second year after founding the firm.

In summary, the [RDD](#) results are consistent with those of the [IV](#) approach. Longer [PBD](#) leads to an increase in actual unemployment duration ([ABD](#)), which then leads to a decrease in subsequent startup success as measured in terms of [FTE](#) employment growth.

### Differences-in-Differences (DID) Approach

The [DiD](#) approach exploits the reform-induced changes in the potential benefit duration ([PBD](#)) (similar to [Cottier et al. 2019](#)) estimating the equation:

$$y_{it} = \alpha + \beta * Treated_{it} + \gamma * After_t + \delta * (Treated * After)_{it} + X_{it} + \varepsilon_{it} \quad (1.6)$$

where  $\alpha$  is a constant,  $X_{it}$  is a vector of person-specific controls (gender, nationality, education, managerial skills), and  $\varepsilon$  an error term for each individual  $i$  in month  $t$ . The  $\delta$  term indicates the causal reform effect of [PBD](#) on our outcomes of interest  $y$ , the actual benefit duration, the motivation for starting up, and the subsequent firm outcomes.

In the [DiD](#) setting, we exploit the 2006 reform. Thus,  $After_t = 1$  is a dummy indicating if an individual becomes unemployed after February 2006. The Treatment-Group consists of workers entering an [UI](#) spell at an age equal to or higher than 45 years, whereas the Control-Group consists of those younger than 45 when claiming [UI](#) benefits. The reform effect measures the treatment of reducing [PBD](#) (and thus [ABD](#)) by at least 3 months.

## Differences-in-Differences (DiD) Results

Our results for the differences-in-differences (DiD) strategy are summarized in [Table I.2](#). Our DiD results capture the causal effect of potential benefit duration (PBD) on actual unemployment duration (ABD) and consequently of the decrease of unemployment duration on the motivation for starting up, as well as subsequent firm outcomes.

The first column of [Table I.2](#) shows that a reduction of at least three months in PBD significantly reduces actual unemployment duration by around 3.6 months. This reconfirms that there is a positive causal relationship between potential and actual benefit duration, and not only with respect to re-employment. Our findings suggest that the translation from potential into actual unemployment duration may be stronger with respect to reductions than increases in the PBD. The results for other outcomes are not statistically significant in the DiD setting when considering all sectors (columns 5/6 in [Table I.2](#)). Focusing again on the non-manufacturing sector as robustness check, [Table I.3](#) reconfirms our findings. As before, the effects now appear to be measured more precisely. In conclusion, the DiD results support our main findings that reducing PBD leads to a statistically significant reduction in actual unemployment duration and to statistically significantly higher sales growth after two years of business activity.

### 1.4.3 Discussion of Results and Mechanisms

Our empirical results based on three different estimation methods (IV, RDD, DiD) suggest a number of conclusions. First, longer PBD increases actual UI duration for those unemployed individuals who start a firm. Hence, our results document that what prior literature has established for individuals transitioning from unemployment to employment (e.g. [Schmieder & von Wachter 2016](#)) also holds for individuals transitioning from unemployment to self-employment. In terms of size, our estimated duration elasticity is slightly above 0.6<sup>14</sup> and thus a bit higher than what recent estimates focusing on transitions from unemployment to paid employment and on increases in PBD suggest.<sup>15</sup> However, our UI duration elasticity estimate corresponds to the median of 0.53 which can be calculated based on the estimates of 18 studies that estimate the UI duration elasticity with respect to employment (cf. Appendix, Table 2 in [Doris et al. 2018](#)).<sup>16</sup>

<sup>14</sup>For the OLS regression, we find a duration elasticity of about 0.48; for the IV strategy, we get a duration elasticity of around 0.67; for the RDD strategy, we find a duration elasticity of around 0.6; and for the DiD strategy, we find a duration elasticity of around 1. Note that differences in results across the different estimation strategies are to be expected: the RDD measures the local average treatment effect, whereas the DiD derives the average treatment effect. Our IV strategy constitutes a compromise of RDD and DiD. Thus, our IV results are expected to be between RDD and DiD. Finally, the fact that IV estimates are larger than OLS estimates shows that measurement error which would lead to downward bias is limited in our data set.

<sup>15</sup>[Le Barbanchon et al. \(2019\)](#) find 0.3 for France, [Nekoei & Weber \(2017\)](#) find 0.016 for Austria or [Schmieder et al. \(2016\)](#) 0.15 for Germany (in the period before 2004), but they all focus only on transitions from unemployment insurance into employment. However, for a decrease in UI generosity [Doris et al. \(2018\)](#) find larger effects. Furthermore, they provide an overview of studies with higher estimates.

<sup>16</sup>Moreover, [Farber et al. \(2015\)](#) suggest that the extensive margin of UI extensions is rather negligible.

Second, longer **PBD** (via longer actual unemployment duration (**ABD**)) increases the fraction of *pushed* entrepreneurs. More unemployed individuals seem to literally escape unemployment by becoming self-employed out of self-reported *necessity* instead of an *opportunity*-driven motivation. Finally, we find overall consistent but not always statistically precisely measured evidence that longer **PBD/ABD** reduces the subsequent success of firms that are started out of unemployment (in terms of sales/employment growth).

The estimated (causal) effects of longer **PBD** on startup motivation and success can be driven by different mechanisms. On the one hand, longer **PBD** can be expected to change the behavior of unemployed individuals, thereby leading to a different composition of founders that decide to start a firm out of unemployment (*composition effect*). On the other hand, by incentivizing longer actual unemployment duration (**ABD**), longer **PBD** could alter the success potential at the individual level (*individual-level duration effect*). For instance, longer actual unemployment duration may lead to losses of financial, social, and human capital, e.g. through losses of professional contacts, stigmatization effects, or depreciation of skills and knowledge.

We attempt to assess the potential influence of both mechanisms by analyzing changes in the composition of our sample of previously unemployed founders over time. We repeat this exercise analyzing changes in the composition of a reference group of previously not unemployed founders over time. Thereby, we focus on comparing both groups across two points in time, before and after the **UI** reform of 2006 (our main source of exogenous variation in **PBD**). In **Table 1.17**, we provide t-tests for before/after reform comparisons of our main explanatory variables and an additionally added broader indicator of founder quality: the average daily wage in employment within five years before starting up (capped at the social security contribution ceiling). We add this measure to assess the potential influence of unobserved factors that we do not control for in our models.

Looking at all unemployed founders in our regression sample that are in the age-based treatment group (two left panels), we observe almost no significant changes in composition before or after the reform. Consistent with the reform and our estimation results, the average **ABD** of treated founders increases significantly. Moreover, significantly more founders receive subsidies by the Federal Employment Agency in the period after the reform (we control for these subsidies in our regressions). Most notably, the (statistically) insignificant but sizable increase in average founders' pre-unemployment wages after the reform is in line with a composition mechanism of "better" founders due to lower **PBD**. The fact that we find smaller effect sizes for the reference group of previously not unemployed founders (right panels of **Table 1.17**) points towards the possibility of *composition effects* induced by the reform on the pool of unemployed founders. Hence, *composition effects* could be one mechanism behind our results but do not seem to be their main driver.

As a further test, we re-estimate our main OLS and **IV** models without any control variables (see [Tables I.4](#) and [I.5](#) as well as [Tables I.6](#) and [I.7](#)). This allows us to assess whether including covariates, which should substantially reduce the impact of *composition effects*, affect our point estimates for changes in **PBD**. When estimated without control variables, all point estimates for changes in **PBD** or **ABD** remain very similar. Hence, the test does not suggest strong reform-induced composition changes in unemployed founders as a driver of our results. This assessment is supported by the summary statistics in [Table 1.2](#). Differences in human capital between unemployed founders and the reference group of previously employed founders seem more sizeable than the small differences between all unemployed founders and unemployed founders with above median unemployment duration. Hence, it is unlikely that these differences over the unemployment duration explain our regression results.

In summary, our data indicate the existence of both *composition* and *individual-level duration* effects that are induced by the **UI** policy reforms. Of the two, our data rather suggest reform-induced *individual-level duration effects* to be the main mechanism explaining the estimated results on startup success. For instance, access to credits may deteriorate with longer unemployment duration and financially constrain the growth potential of firms started out of unemployment (in line with recent findings of e.g. [Caliendo et al. 2019](#)). However, future research should investigate the mechanisms in more details and quantify the relative importance of *composition* versus *individual-level duration* effects.

## 1.5 Stylized Theoretical Model

In the following, we present a stylized model to rationalize the empirical findings that we observed in the previous [Section 1.4](#). We conclude this section by discussing policy options in the model ([Section 1.5.2](#)), implications in terms of fiscal externalities ([Section 1.5.3](#)) and general policy implications of our findings ([Section 1.5.4](#)).

### 1.5.1 The Framework

We consider workers who become unemployed in period  $t = 0$  as risk-neutral, provided they stay in unemployment receiving **UI** benefits for duration  $d$ .<sup>17</sup> In each time period (month), they get unemployment benefits  $b_t$  until the maximal potential unemployment benefit duration **PBD** is reached (cf. [Section 1.3.1](#)). Focusing on the case of a two-layer **UI** system, this means that benefits can be defined as  $b_t = \bar{b}$  for  $t \leq \text{PBD}$ , where  $\bar{b}$  is the constant **UI** benefit level which the unemployed individual receives for the entire **UI** spell (until exhausting benefits at the potential benefit duration **PBD**). The **UI** benefits depend on the previous wage; they constitute the replacement rate fraction of the average monthly wage income over the six months before entering unemployment.

<sup>17</sup>The model is in continuous time and the horizon lasts for each worker until retirement time  $T$ .

Then,  $b_t = \tilde{b} < \bar{b}$  for  $t > PBD$ , where  $\tilde{b}$  can be interpreted as Germany's existential minimum assistance “Arbeitslosengeld II” that is independent of previous contribution months and lower than the wage-dependent **UI** benefits ( $\bar{b}$ ). Without loss of generality, this amount is the same for all eligible claimants.<sup>18</sup>

We assume that each individual has a latent entrepreneurial ability  $\theta \sim G(\theta)$ , where  $G(\cdot)$  is a normal density function. Then in each time period, an unemployed individual has to decide whether to search for employment or to start a firm, i.e. to become self-employed. Let  $V_u^e$  be the value function of an unemployed individual searching for employment, and  $V_u^{se}$  that of an unemployed individual starting a business. Then, the decision of an unemployed individual is characterized by her value function:

$$V_t^u = \max\{V_{u,t}^e, V_{u,t}^{se}\} \quad (1.7)$$

**Value of Searching for Employment out of Unemployment** Ignoring savings (workers live hand-to-mouth), the value of searching for employment when the individual is unemployed can be characterized by:

$$V_{u,t}^e = b_t - \psi_t(s_t) + \beta \left\{ p_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} V_{t+1}^e(w_{t+1}) dF(w_{t+1}) + [p_t F(\phi_t) + (1 - p_t)] V_{t+1}^u \right\} \quad (1.8)$$

The unemployed individual receives consumption flow utility from benefits  $c_{u,t} = b_t$ <sup>19</sup>, but faces search costs  $\psi_t$ , which, in line with the literature (e.g. [Schmieder & von Wachter 2016](#)), we assume to be a differentiable, increasing and convex function of search effort  $s_t$ . With probability  $p_t = p(s_t, \theta)$  that depends on search effort and (entrepreneurial) ability, the unemployed worker receives a job offer for period  $t + 1$ . Note that in this setting, the individual's optimal behaviour is characterized by a reservation wage  $\phi_t$  above which any wage offer  $w_t \geq \phi_t$  is accepted.<sup>20</sup>

Thus, with probability  $1 - F(\phi_t)$  the offer is accepted and she becomes re-employed, receiving the corresponding expected value of being employed  $V_{t+1}^e$  (see [Equation \(1.9\)](#)). However, with probability  $F(\phi_t)$ , the offer is too low and is rejected. In this case and if the worker receives no offer (with probability  $1 - p_t$ ), she remains unemployed in the next month and receives the next period value of being in unemployment,  $V_{t+1}^u$  ([Equation \(1.7\)](#)). As usual,  $\beta$  is the discount factor of future period returns.<sup>21</sup>

<sup>18</sup>Note that the replacement rate for **UI** benefits ( $\tilde{b}$ ) is 60% for single individuals and 67% for individuals with dependent children. When receiving the existential minimum ( $\tilde{b}$ ), the additional amount received per child on top of the basic minimum approximately corresponds to the general child allowances every parent receives from the German federal state (“Kindergeld”). Thus, the relative drop in income when exhausting **UI** benefits does not vary much per person independent of the family structure and we abstract from this issue for the purpose of this chapter.

<sup>19</sup>Note that  $c_{u,t} = b_t + y_u$  where  $y_u$  could be income from other sources that, if assumed to remain constant over the **UI** spell and exogenously given, would not alter our qualitative conclusions (e.g. support from family members).

<sup>20</sup>Note that the cumulative distribution function  $F(\cdot)$  may depend on the duration of unemployment, for instance, due to depreciation in human capital or (statistical) discrimination or stigma effects, as explained by [Jarosch & Pilossoph \(2019\)](#) and experimental evidence suggests ([Oberholzer-Gee 2008](#)).

<sup>21</sup>One could introduce myopic behavior of agents by changing the discount factor. We abstract from this complication, as we have no empirical evidence for irrational behavior driving our results.

The two-layer **UI** system implies through the parameter  $b_t$  that if an individual stays unemployed, surpassing the potential benefit duration (**PBD**), the outside option will decline to the existential minimum (from  $b_t = \bar{b}$  to  $b_t = \tilde{b} < \bar{b}$ ). Thus, a drop in the value function ( $V_{u,t}^e$ ) is to be expected in the month of the unemployment spell when the **PBD** is reached.

**The Value of Being in Employment** is then characterized by:

$$V_t^e = (w_t - \tau) + \beta \{ \lambda_t V_{t+1}^u + (1 - \lambda_t) V_{t+1}^e \} \quad (1.9)$$

An employed worker receives consumption flow utility  $c_{e,t} = w_t - \tau$ , i.e. consumption based on net wage.<sup>22</sup> Variable  $\lambda_t$  is an exogenous separation rate that may vary depending on macroeconomic conditions over time. Then, with probability  $\lambda_t$ , the worker may lose her job and become unemployed again, but with probability  $1 - \lambda_t$  the worker remains employed. As a simplifying restriction, we ignore the option of moving from employment to self-employment but focus on flows from unemployment to self-employment, as this resembles our available empirical setting and is the relevant labor market flow we study.

**Value of entering Self-Employment out of Unemployment** The value for an unemployed individual to become self-employed out of unemployment is characterized by:

$$V_{u,t}^{se} = b_t - \psi_t^{se}(s_t, \theta) + \beta \left\{ p_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} V_{t+1}^{se}(\pi_{t+1}) dF(\pi_{t+1}) + [p_t F(\phi_t) + (1 - p_t)] V_{t+1}^u \right\} \quad (1.10)$$

An unemployed individual evaluating whether to become self-employed faces a similar value function as in the case of searching for employment **Equation (1.8)**. Again, she receives a consumption flow utility in the form of unemployment benefits  $b_t$  and faces search costs  $\psi_t^{se}(s_t)$ . These search costs could be different and more dependent on the individual than in the case of searching for employment, since an individual has to develop an idea, do market research, or find capital instead of writing applications in a more standardized process of looking for paid employment. Furthermore, becoming self-employed is more dependent on one's own skills  $\theta$ . The higher the entrepreneurial ability, the smaller are market-entry search costs. The individual unemployed still faces a reservation wage  $\phi_t$  above which potential profits from self-employment would be accepted. However, if potential profit is too low, the individual may remain in unemployment to look for employment. Otherwise, if profits as self-employed are higher than the reservation wage  $\pi_t \geq \phi_t$ , she will prefer to start up.

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<sup>22</sup>Taxes could be also designed to be proportional taxes  $(1 - \tau)$  without changing the qualitative results.



**Value of Being in Self-Employment** The value of being in self-employment can be characterized by the following value function:

$$V_{ut}^{se} = \pi_t(\theta) + sub_t + \beta \{ \gamma(\theta) V_{t+1}^u + (1 - \gamma(\theta)) V_{t+1}^{se} \} \quad (1.11)$$

A self-employed person earns profits  $\pi_t(\theta)$  (net of a startup cost) and may get a subsidy  $sub_t$ . Thereby, the returns  $\pi_t(\theta)$  are assumed to be increasing in entrepreneurial skills ( $\frac{\partial \pi_t(\theta)}{\partial \theta} > 0$ ) reflecting that, for instance, the quality of successful business ideas may increase with  $\theta$ . Similarly, the probability that the startup fails,  $\gamma(\theta)$ , is assumed to be decreasing in entrepreneurial ability,  $\frac{\partial \gamma(\theta)}{\partial \theta} < 0$ , reflecting e.g. that better business ideas are less likely to produce failure. Thus, with probability  $(1 - \gamma(\theta))$  the startup survives and with probability  $\gamma(\theta)$ , the founder has to return to unemployment,  $V_{t+1}^u$  Equation (1.7).

**The Effect of Unemployment Duration on Value Functions** The unemployed workers decision problem Equation (1.7) is to maximize expected utility between the value of moving from unemployment to employment and the value of becoming self-employed out of unemployment. See Appendix I.4 for the derivation of results.

First, the **value function of moving from unemployment to employment** can be characterized by Equation (1.8) where  $V_{t+1}^e$  is characterized by Equation (1.9). It is important to note that given a fixed level of (entrepreneurial) ability  $\theta$ , the value function  $V_{u,t}^e$  features negative **duration dependence**  $\frac{\partial V_{u,t}^e}{\partial d} | \theta < 0$ . There are two main sources for **UI duration dependence**: the search effort may vary over the unemployment duration and over time the benefit levels are decreasing (at least once reaching **PBD** with the drop to the existential minimum).<sup>23</sup> The accepted job offer's value depends on both search effort determining the job offer arrival rate  $p(s_t, \theta)$  and on the re-employment wage.<sup>24</sup>

We derive the optimal search intensity and reservation wage paths in order to observe how these variables react to an increase in  $d = \text{PBD}$ . We find that the reservation wage is positively correlated to the unemployment duration, i.e.  $\frac{\partial \phi_t}{\partial d} > 0$ . The search intensity reacts negatively to an increase in potential unemployment duration, i.e.  $\frac{\partial s_t}{\partial d} < 0$ . This implies that an individual will prefer to stay longer unemployed when **PBD** is increased. Negative **UI duration dependence** has been shown to be an empirically robust finding regarding re-employment outcomes, even if there is not yet a consensus concerning the welfare implications for post-unemployment wages (e.g. Schmieder et al. 2016, Schmieder & von Wachter 2016, Nekoei & Weber 2017).

<sup>23</sup>Nekoei & Weber (2017) show that a directed search model incorporates these two sources of *duration dependence* and includes the random search McCall-style model that we presented as a special case. Moreover, they reveal that selectivity may be positively, and *duration dependence* negatively affecting re-employment wages.

<sup>24</sup>Note that there have been different theories proposed to explain this finding. They include, for instance, first, that human capital or job-specific skills may decay over the non-employment spell. Second, statistical discrimination has been suggested for explaining this finding because it implies that less able persons remain longer unemployed. Third stock-flow matching could also explain *duration dependence* as it implies that those entering unemployment and not quickly finding a match become increasingly dependent on the inflow of new posted vacancies (and flow variables are quantitatively smaller than stock variables).

Second, the **value function of moving from unemployment to self-employment** can be characterized by Equation (1.10) where  $V_{t+1}^{se}$  is characterized by Equation (1.11). Holding entrepreneurial ability fixed, this value function is also dependent on unemployment duration  $d$ , that is  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta < 0$ , but in absolute terms, it is smaller than  $\frac{\partial V_{ut}^e}{\partial d}$ , ie.  $\frac{\partial V_{ut}^e}{\partial d} | \theta < \frac{\partial V_{ut}^{se}}{\partial d} | \theta < 0$ . We derive this by exploiting the definitions of  $V_{u,t}^e$  and  $V_{u,t}^{se}$ . By assuming that the value of leaving unemployment in the next period depends on average on the present value of the reservation wage, we can write  $V_{u,t+1}^e = \frac{1}{\rho} \phi_t$ . In the case of self-employment, the latter definition has to be extended in the following way:  $V_{t+1}^{se} = \frac{1-\gamma(\theta)}{\rho} \phi_t$ , because one can only become self-employed with a probability of  $[1 - \gamma(\theta)]$ .

Deriving the optimal reservation wage and search intensity paths, we show that the optimal search intensity for business opportunities is less dependent on unemployment duration  $d$ . Thus, the negative unemployment *duration dependence* for becoming self-employed is smaller than in the case of searching for employment. This reflects the idea that self-employment can be interpreted as an alternative professional activity more dependent on one's own skills than on the labor market conditions and is thus approximately independent of unemployment duration (compare Appendix I.4). Moreover, the search process for self-employment is different from the search for regular employment.

Now it is possible to rationalize our empirical results by analyzing qualitatively how the value functions for becoming employed  $V_{ut}^e$  (Equation (1.8)) and self-employed  $V_{ut}^{se}$  (Equation (1.10)) evolve with unemployment duration  $d$  and how this influences an unemployed individual's decision, given her entrepreneurial ability. Figure 1-6 illustrates that the unemployed individual will prefer to find a job provided that the value function for searching employment  $V_{ut}^e$  is above the value function for becoming self-employed  $V_{ut}^{se}$ . Vice versa she prefers to start a business when  $V_{ut}^{se} > V_{ut}^e | \theta$ . The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line shows  $\frac{\partial V_{ut}^e}{\partial d} | \theta < \frac{\partial V_{ut}^{se}}{\partial d} | \theta < 0$ . Moreover, the vertical black line marks the **PBD**; at this point of unemployment duration, the red line drops by  $x = \bar{b} - \tilde{b}$  because **UI** benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  (compare Appendix I.3). Thus, the stylized model suggests that potential benefit duration can determine the composition of the self-employed who transition from unemployment. This becomes apparent when analyzing how unemployed individuals behave in this model.

First of all, holding everything fixed but changing entrepreneurial ability, we can observe the following. As Figure 1-7 illustrates, some high ability individuals may always decide to become self-employed once unemployed, i.e. once having to search for a new post-unemployment labor market status. Instead, Figure 1-8 shows that certain low-skilled unemployed persons would never decide to become self-employed, but rather search for employment. Note, that we are thinking about individuals that have been employees and after falling into unemployment start to consider self-employment as an alternative to re-employment. Thus, they may only start to think about their entrepreneurial ability once they become unemployed.



As Figure 1·9 illustrates, our model explains how the government can influence the composition of unemployed individuals that decide to become self-employed out of unemployment via setting the length of potential benefit duration (PBD). The longer one is unemployed before moving to self-employment, the lower her  $\theta$  and thus the lower subsequent firm performance appears to be (i.e. the composition changes). Moreover, if we considered negative *individual-level duration effects* to be the main response to a longer actual unemployment duration (that was induced through longer PBD), we would reach similar qualitative conclusions. In other words, *composition* and *individual-level duration* effects or a mixture of both, would be in line with the empirical evidence i.e. that PBD positively affects actual benefit duration and through the latter negatively affects the motivation to become self-employed as well as subsequent startup success.

Finally, as Figure 1·10 shows, our model can also rationalize our results for the case that unemployment duration would not additionally harm potential self-employment outcomes i.e. in the case of zero UI *duration dependence* for self-employment outcomes. In this case, the empirical result of the subsequent startup success declining over the unemployment duration of a founder may also be rationalized with a horizontal value function  $V_u^{se}$  that is independent of  $d$ . To summarize, Figure 1·11 demonstrates that the government can change startup success by setting the PBD.

### 1.5.2 Further Policy Options in the Model

**Early Re-training for Employment** Figure 1·12 shows that, given the same maximal PBD, early re-training can reduce the rate at which  $V_u^e$  declines with actual unemployment duration. Thus, for a fixed PBD, retraining may improve welfare, maintaining consumption smoothing and general matching considerations for unemployment to employment transitions. This could reduce the number of *necessity* founders via slowing the negative UI *duration dependence* in  $V_u^e$  that is itself causally influenced by PBD.

**Targeted Subsidies for Self-Employment** As Figure 1·13 illustrates, targeted subsidies to unemployed workers that may have revealed some entrepreneurial skills, e.g. by a business plan, would increase  $V_u^s$ , thus, the post-unemployment startup success probability. In that way, they could ease the decision of unemployed individuals with promising ideas to stop searching for employment, while focusing on preparing their startup. Consequently, this could reduce their time in unemployment (hence the fiscal externality).<sup>25</sup>

<sup>25</sup>Early training for self-employment (relaxing the assumption of entrepreneurial types, such that some skills may be trained through external support) could increase  $V_u^s$ . Thus, the post-unemployment startup success probability could be increased, e.g. by coaching them in setting up better business plans.

### 1.5.3 Implications for Fiscal Externality

Thinking about optimal **UI** benefit duration, one has to consider the social costs of changing the potential benefit duration which is the so called fiscal externality it creates (Lawson 2017). That is, in the spirit of the Baily-Chetty framework (e.g. Chetty 2009, Kroft & Notowidigdo 2016), optimal **UI** benefit duration should balance the welfare benefits of additional insurance created that helps to smooth overall consumption with the social cost of extending **PBD**. The latter is captured by the fiscal externality (the effect on government budget).

To illustrate the role of taking self-employment out of unemployment into account, let us consider an example. In her last pre-unemployment job, a worker earns wage  $w$  and pays taxes  $\tau$ .<sup>26</sup> The worker enters unemployment in time period  $T_0$ . In line with our results, extending the potential benefit duration **PBD** would induce longer actual unemployment of  $(T - T_0)$  periods. In time period  $T$ , the unemployed individual becomes self-employed. If the person becomes a *necessity* entrepreneur who actually does not want to become self-employed but only becomes self-employed to escape unemployment, her profits  $\pi_s$  could be lower than the pre-unemployment wage  $w$  (as proxy for the hypothetical re-employment wage) during her self-employment spell (lasting from period  $T + 1$  until time period  $S$ ). If in addition the *necessity* entrepreneur failed at time  $S$  and would drop back into unemployment from time  $S + 1$  onward, this would produce costs of forgone tax revenues  $\tau\pi_s$  and the benefits paid during unemployment ( $b$ ). Formalizing the example of a *necessity* entrepreneur, we get a formula for the fiscal externality:

$$FiscalExternality = (\tau w + b)(T - T_0) + \sum_{s=T+1}^S \tau(w - \pi_s) + \sum_{s=S+1}^{\infty} (\tau\pi_s + b)D_s \quad (1.12)$$

The first term is the standard duration effect, which imposes a negative fiscal externality in the case of limited **UI** duration. In fact, longer non-employment duration implies that the government forgoes potential tax revenue ( $\tau w$ ) and in addition has to pay for the unemployment insurance expenditures  $b$  over the non-employment spell of the unemployed worker. By increasing **PBD**, this effect would increase the negative fiscal externality, not only for those who then become employed (Schmieder et al. 2016), but as we showed also for those who start a business out of unemployment, because in both cases the **PBD** is positively linked to actual unemployment length.

The second term takes into account the effect of the unemployment insurance on self-employment performance for the government budget. In the given example, when the profits as self-employed are below pre-unemployment wages ( $\pi_s < w$ ) [as proxy for hypothetical re-employment wage], the negative fiscal externality would increase.<sup>27</sup>

<sup>26</sup>Without loss of generality, we just consider proportional taxes and follow Lawson (2017) in assuming that the fiscal externality of social security programs works through labor income taxes.

<sup>27</sup>Note that this case is plausible even at low income levels. Regular wages are usually bound by minimum wages, whereas the corresponding earnings from self-employment have no lower bound.

However, in theory this term could also decrease the overall negative fiscal externality: if  $\pi_s > w$ . This might be the case for *opportunity* entrepreneurs who have a good business plan and may be better off compared to pre-unemployment wages or other re-employment options.<sup>28</sup>

The last term expresses the extra cost if the self-employed fails and subsequently has to return to unemployment. In that case a second-order duration effect consisting of forgone tax revenue and potential benefit payments could further increase the fiscal externality.

In summary, taking the effect of longer potential benefit duration (PBD) on self-employment performance into account may change the overall fiscal externality of the unemployment insurance. This in turn could alter optimal UI considerations. For instance, if PBD pushed many unemployed individuals into *necessity* entrepreneurship and this caused the negative fiscal externality to grow, as in the given example, this may imply a decrease of optimal PBD. Given that the optimal UI literature usually only considers the transition from unemployment into paid employment (ignoring transitions into self-employment), if at all, only the first standard duration term in Equation (1.12) applies. Thus, it is important to consider the impact of PBD on self-employment and the associated fiscal externality when it comes to the optimal design of the unemployment insurance.

#### 1.5.4 Policy Implications

First, our results could have implications for the design of optimal UI policy. The previous Section 1.5.3 shows that the potential UI benefit duration (PBD) may increase the fiscal externality through its effect on self-employment performance. In abstracting from the fact that unemployed individuals can also choose to enter self-employment instead of employment, the literature and politics have neglected this effect. Too much selection into self-employment due to necessity may imply high social costs. Thus, the general UI system should design optimal PBD in a way that considers both employment and self-employment outcomes. Moreover, this could improve our insights into the so-called value of non-employment that itself is important for the results of many wage bargaining models (Jäger et al. 2019).

Second, since economic trends induced by digitization may lead to more startups in the future, thinking about the design of social safety nets, in particular with respect to unemployment insurance for self-employed individuals, may become increasingly relevant. In that respect, the results for Germany, with a low overall unemployment rate, may be considered to be rather lower bound estimates. Thus, this chapter of my doctoral thesis shifts attention to an important discussion of how one should best design social safety nets for self-employed individuals.

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<sup>28</sup>In fact, if this positive effect dominated, this would correspond to the positive UI wage effect as suggested by Nekoei & Weber (2017) for those who start a business instead of finding re-employment.

Finally, the findings in this first chapter of my dissertation may also be relevant in the evaluation of active labor market policies (ALMPs). This is because ALMPs can be interpreted as measures that usually involve extending **PBD** and providing subsidies that correspond to **UI** benefits. Often, those active labor market policies target the long-term unemployed. In the light of our results, this raises questions as to what extent current policies for those unemployed individuals are desirable. Our results indicate that interventions should not be measures of a last resort but instead target the unemployed individuals early during their unemployment spell. In general, more investment in early retraining and well-targeted startup subsidies for unemployed individuals who have sustainable business ideas could improve who decides to start a firm. This could reduce fiscal externalities and improve social welfare.

## 1.6 Conclusion

This first chapter of my doctoral thesis addresses the question of how the potential unemployment insurance (**UI**) benefit duration (**PBD**) affects the actual unemployment duration (**ABD**) before unemployed individuals become self-employed, as well as their motivations to become self-employed and their outcomes as entrepreneurs. While existing literature has addressed how **UI** policies affect the unemployment duration and re-employment wages of those transitioning to dependent employment, we are the first to address this issue in the context of transitions to self-employment by creating a new representative dataset on founders in Germany. Since active labor market policies, which incentivize mainly long-term unemployed individuals to become self-employed, are commonly used as policy measures to fight unemployment, understanding the effects of the design of **UI** policies on self-employed seems to be highly relevant.

Using instrumental variables methods (**RDD** and **DiD** for robustness), we identify the causal effects of the **PBD** on entrepreneurial outcomes by exploiting reform and age-based exogenous variation in **PBD** within the German **UI** system. We find that in a sample of previously unemployed founders, longer **PBD** leads to longer actual unemployment duration and, through the latter, increases the propensity that unemployed individuals are *pushed* into self-employment (out of *necessity*), as opposed to starting a firm because of a business *opportunity*. Moreover, longer unemployment duration is associated with worse entrepreneurial performance in terms of both employment growth and sales.

This net negative (overall) causal relationship can be rationalized by a mix of both an *effect on the composition* of startups out of unemployment, and an *individual-level duration effect* on the founders over the **UI** spell. In a stylized formal model, we show how both mechanisms can explain why the government's change in **PBD** causally generates our observed findings for firms started out of unemployment.

Extensions of our empirical analyses show little changes to the composition of unemployed founders over different **UI** policy regimes and therefore suggest that our results are at least partly driven by **UI** duration policy affecting the individual-level entrepreneurial potential. A consistent explanation for this finding is that individuals' financial, social, and human capital depreciates in unemployment. However, an exact empirical derivation of the quantitative importance of the different mechanisms behind our findings is beyond the scope of this doctoral thesis. Analogously to the literature on **UI** policy effects on individuals transitioning to dependent employment (e.g. [Schmieder & von Wachter 2016](#)), additional research, data, and methods are needed to better assess the contributions of different mechanisms to the overall policy effect. Independent of the exact mechanism, our results allow us to conclude that there exists a causal effect from **UI** policy with respect to **PBD** via actual unemployment durations to startup motivation and startup success.

Given the current lack of evidence regarding the role of **PBD** on startup success (or self-employment in general), our results are thus of strong relevance from a public policy perspective. They show that it is important to consider self-employment as a post-unemployment outcome in typical optimal **UI** models which are based on the sufficient statistics approach following the Baily-Chetty model ([Chetty 2009](#), [Landaís et al. 2018](#)). Ignoring entrepreneurship out of unemployment likely leads to underestimating fiscal externalities. For instance, **UI** policy could trigger firm creations by low performing *necessity* entrepreneurs whose tax revenues are comparably low or who may cause extra costs for society when returning to unemployment. Rather than pushing the long-term unemployed into self-employment, **UI** policies might therefore be more profitable when targeted at early re-training for those who would otherwise become self-employed out of *necessity* later. Our results are particularly relevant for all countries with generous **UI** benefit duration and countries granting extended **UI** benefit duration for founders starting a business out of unemployment. Due to its relatively low unemployment and self-employment rate levels, our results for Germany may be lower bound estimates for other countries.<sup>29</sup>

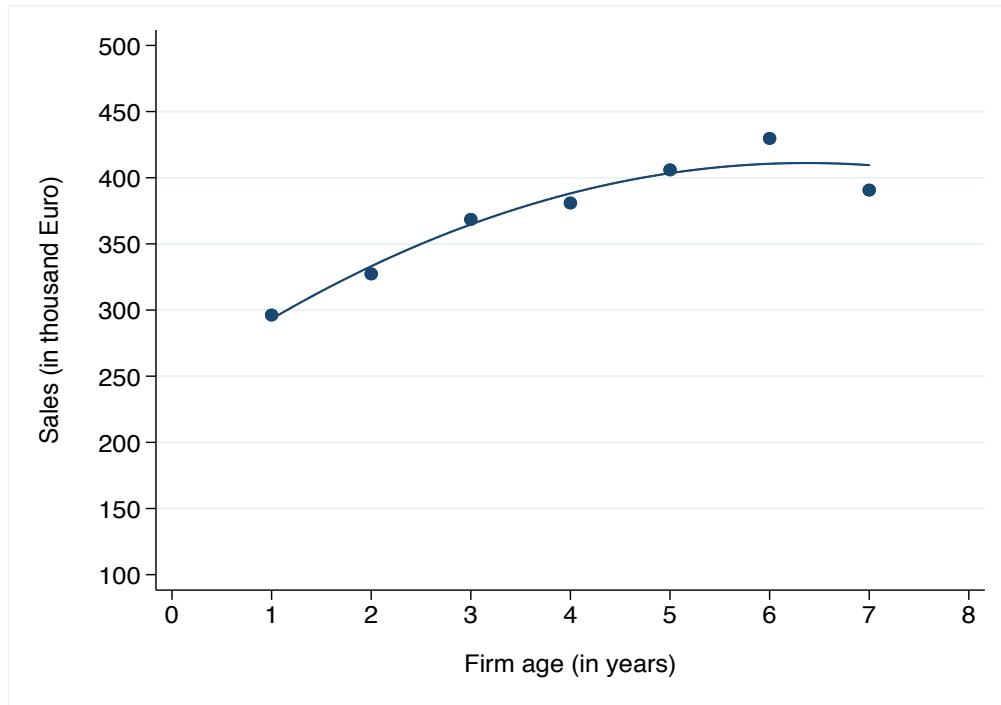
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<sup>29</sup>[Camarero Garcia & Hansch \(2020\)](#) investigate the role of **UI** benefit levels on self-employment for the case of Spain. This working paper, which corresponds to [Chapter 2](#), complements the picture because only the results of both **PBD** and **UI** benefit levels on self-employment reveal the total effect of **UI** on the transition channel from unemployment to self-employment.

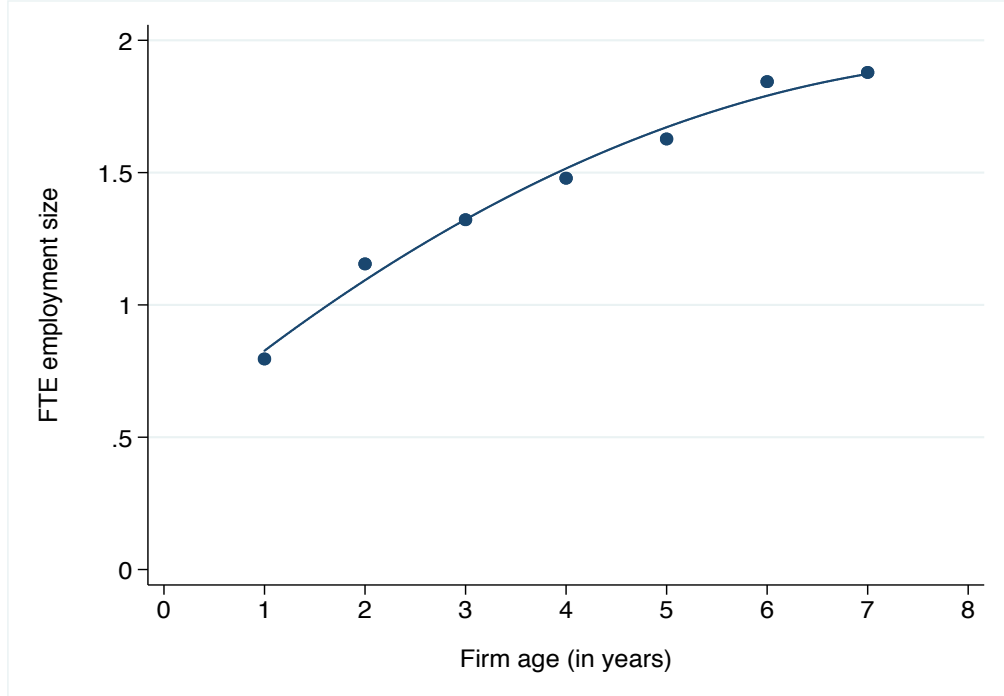
## 1.7 Figures

**Figure 1-1: Firm Outcomes in Years after Foundation for All Founders**

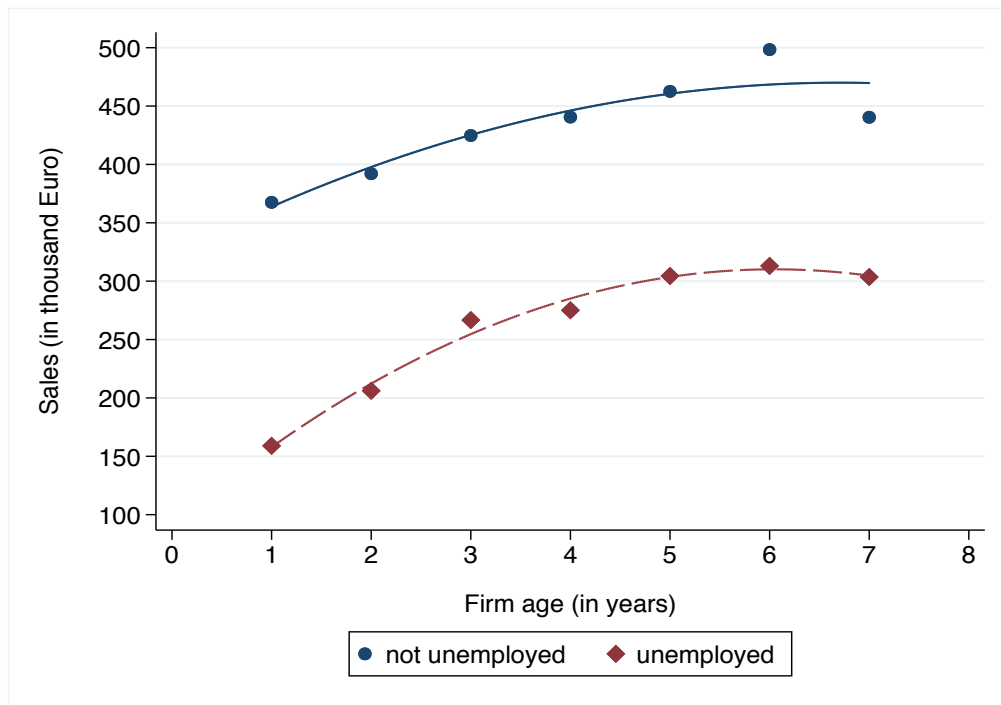
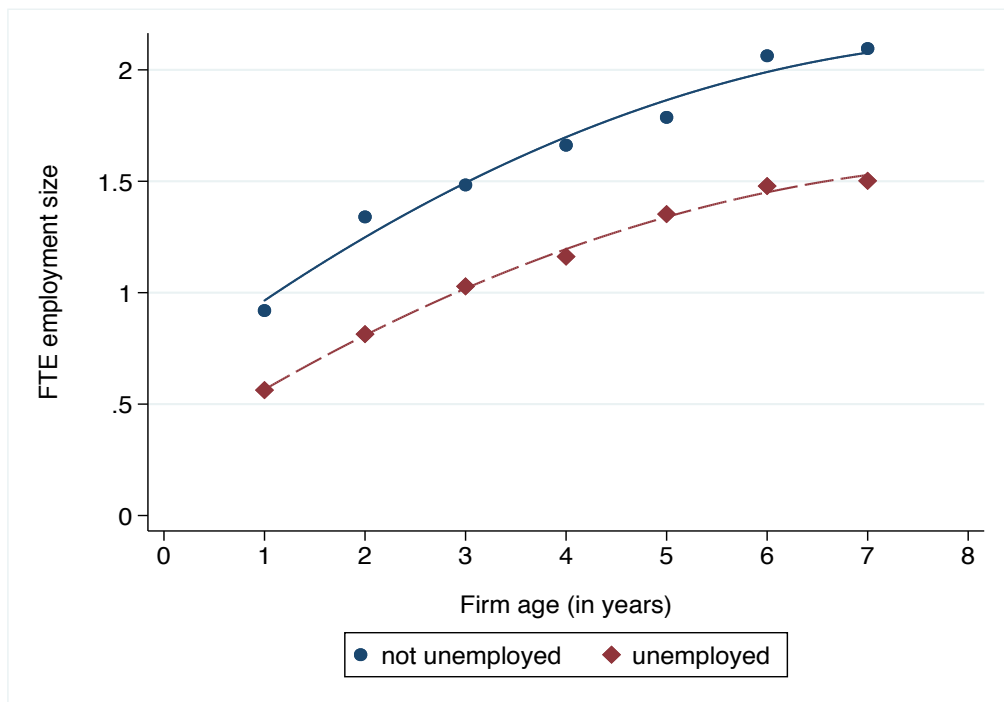
(a) Sales in EUR



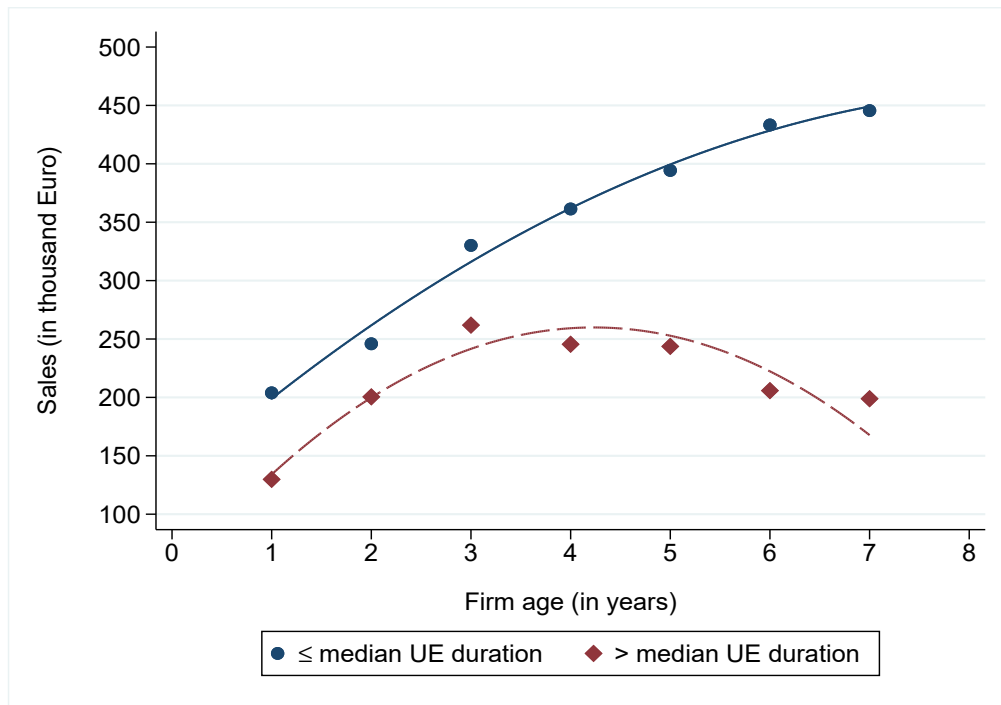
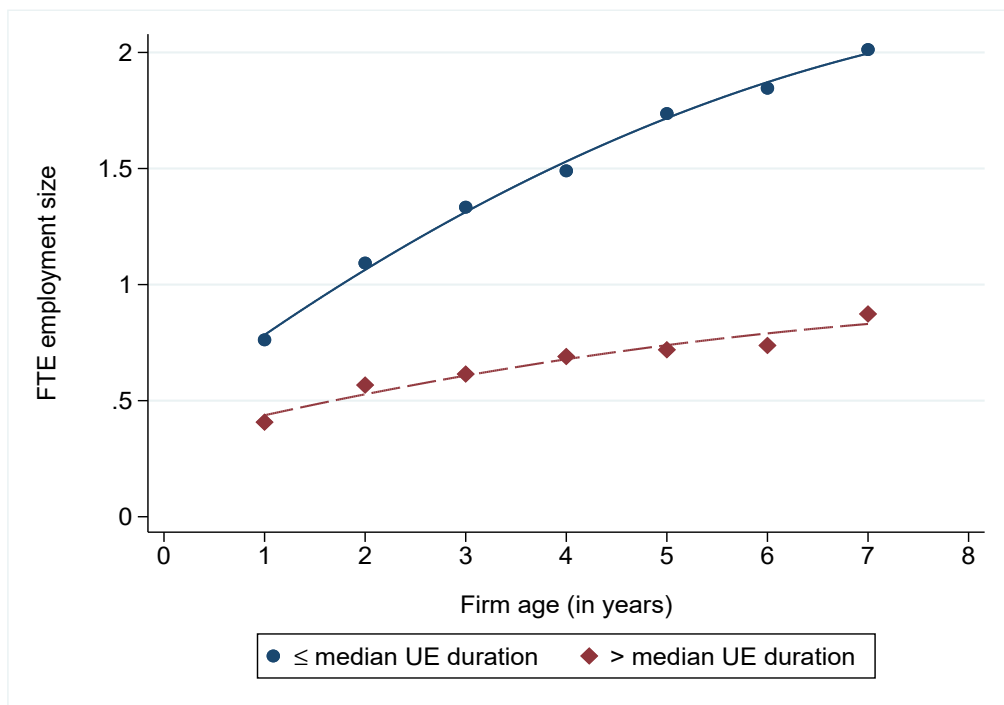
(b) Full-Time Equivalent Employment



*Notes:* The Figure shows firm outcomes of non-team founders aged 35-65 (analogous to the definition of our main estimation sample) in years after foundation. We see the outcomes of startups in terms of sales per year and full-time equivalent employment based on 5,250 (sales) and 5,850 (employment) startups established between 2005 and 2011 from our linked dataset (see [Section 1.2](#)). Firms usually stay in the panel for seven years but can drop out if they fail or refuse to take part in more than two consecutive years. Thus, less firms are observed in year seven compared to year one after starting up.

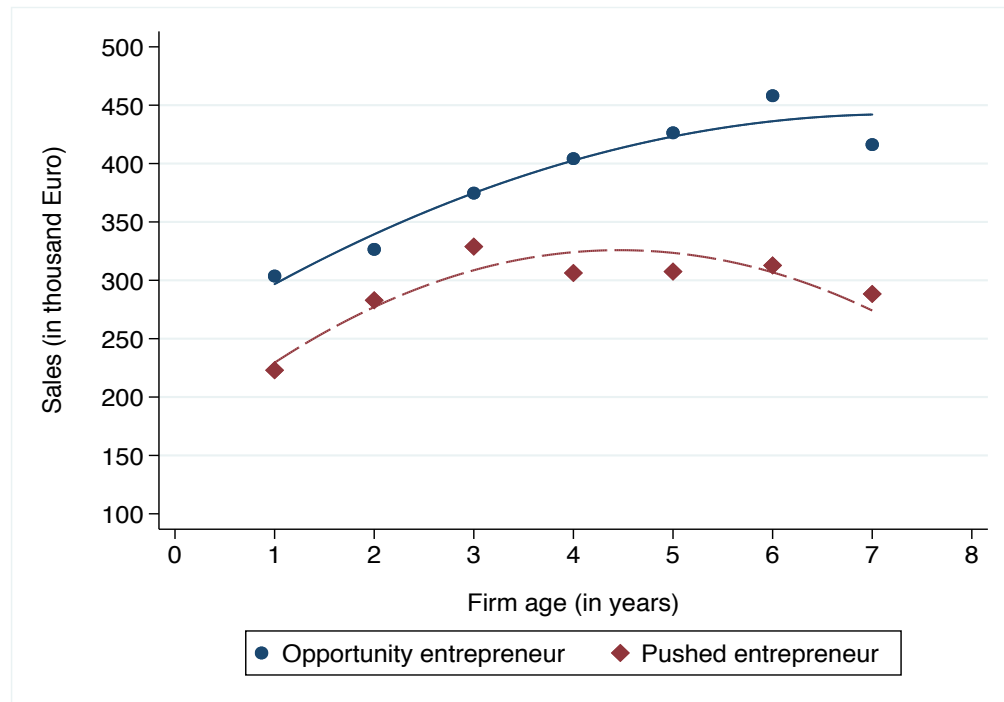
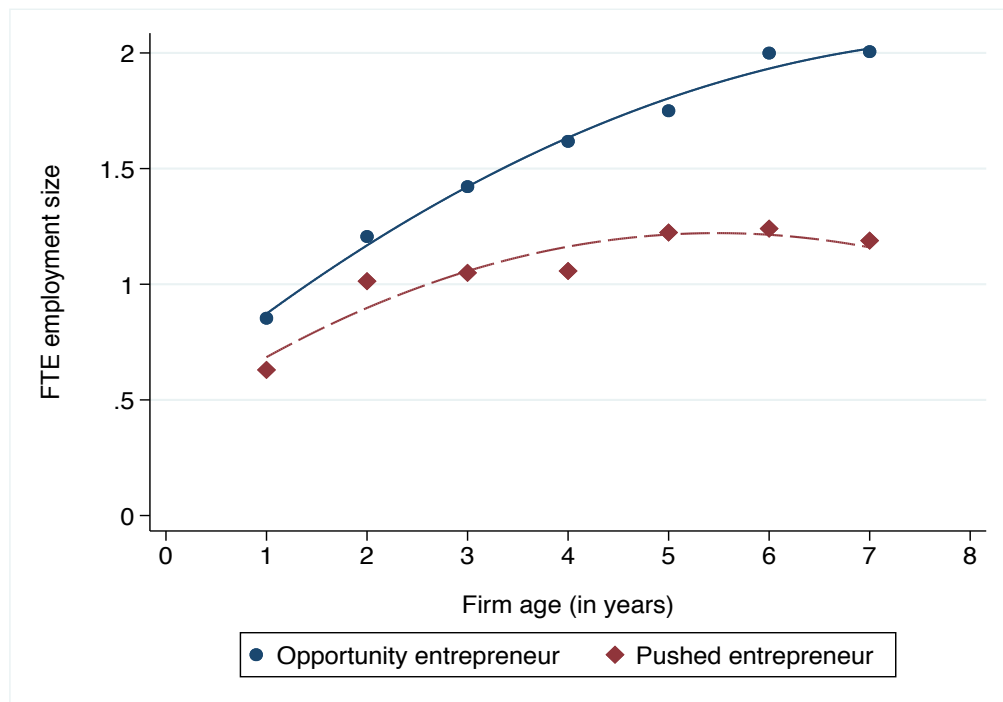
**Figure 1.2: Firm Outcomes in Years after Foundation by Previous Employment Status****(a) Sales in EUR****(b) Full-Time Equivalent Employment**

*Notes:* The Figure shows firm outcomes of non-team founders aged 35-65 (see the definition of our main estimation sample in [Table 1.2](#)) in years after foundation split by the previous labor market status of the founder (not unemployed or unemployed). We cover startups established between 2005 and 2011 from our linked dataset as described in [Section 1.2](#).

**Figure 1.3:** Firm Outcomes in Years after Foundation split by Median Unemployment Duration**(a)** Sales in EUR**(b)** Full-Time Equivalent Employment

*Notes:* The Figure shows firm outcomes of non-team founders aged 35-65 (see the definition of our main estimation sample in [Table 1.2](#)) with previous unemployment spell in years after foundation split at the medium (actual) unemployment duration. We cover startups established between 2005 and 2011 from our linked dataset as described in [Section 1.2](#).

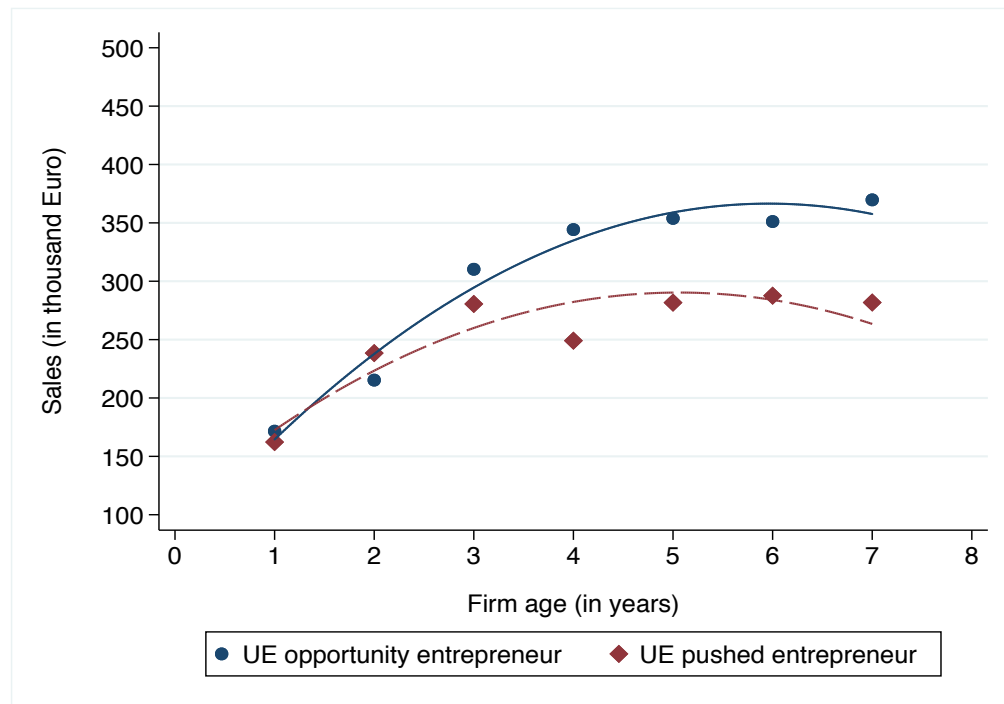


**Figure 1-4: Firm Outcomes in Years after Foundation split by Motivation for Starting Up****(a) Sales in EUR****(b) Full-Time Equivalent Employment**

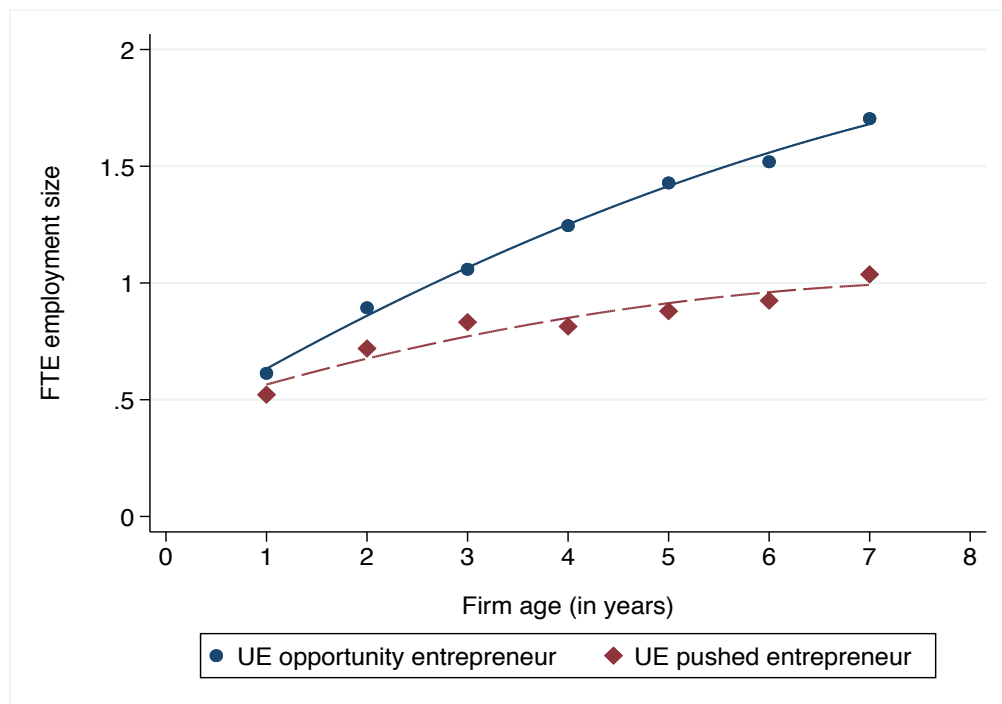
*Notes:* The Figure shows firm outcomes of non-team founders aged 35-65 (analogous to the definition of our main estimation sample) in years after foundation split by self-reported motivation, i.e. *opportunity* vs. *pushed/necessity* driven entrepreneurship. We cover approximately 5,050 (sales) and 5,600 (employment) startups established between 2005 and 2011 from our linked dataset as described in [Section 1.2](#). The notion of using, instead of *necessity*-driven founder, the term *pushed* entrepreneur is best understood by checking the spikes of the exit rate from unemployment into self-employment split by the motivation to start up which is shown in [Figure I-1](#).

**Figure 1-5: Firm Outcomes in Years after Foundation by Motivation for Starting Up out of Unemployment**

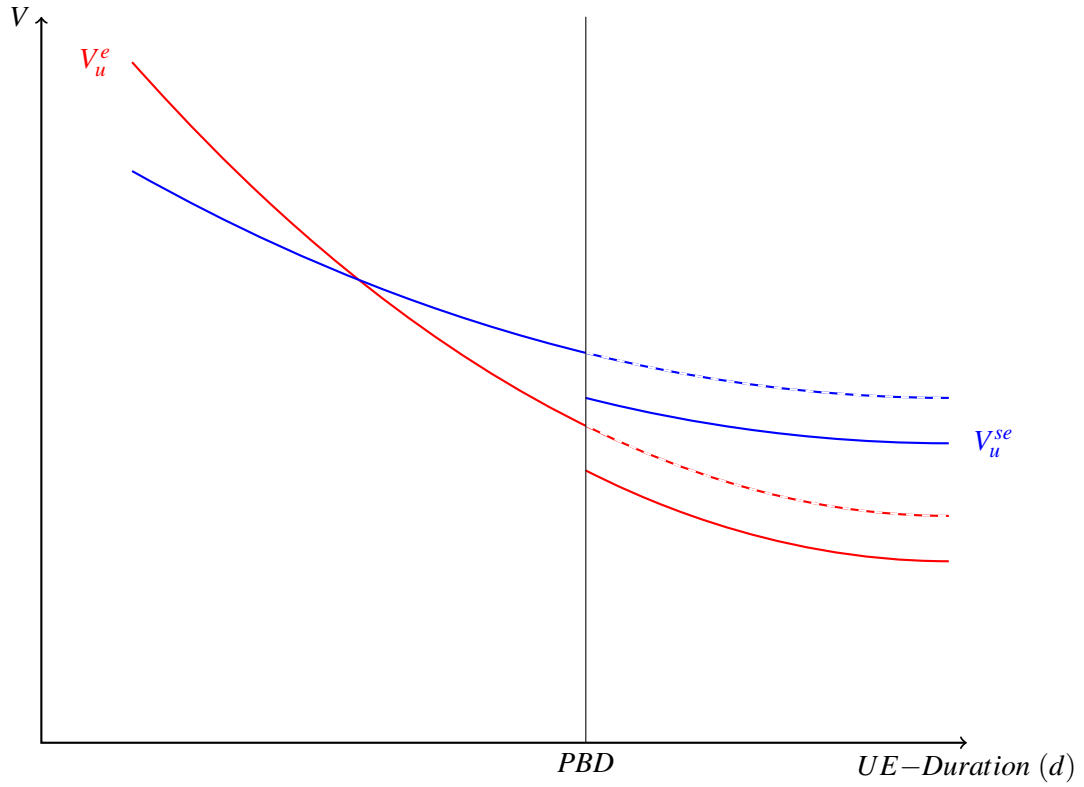
(a) Sales in EUR



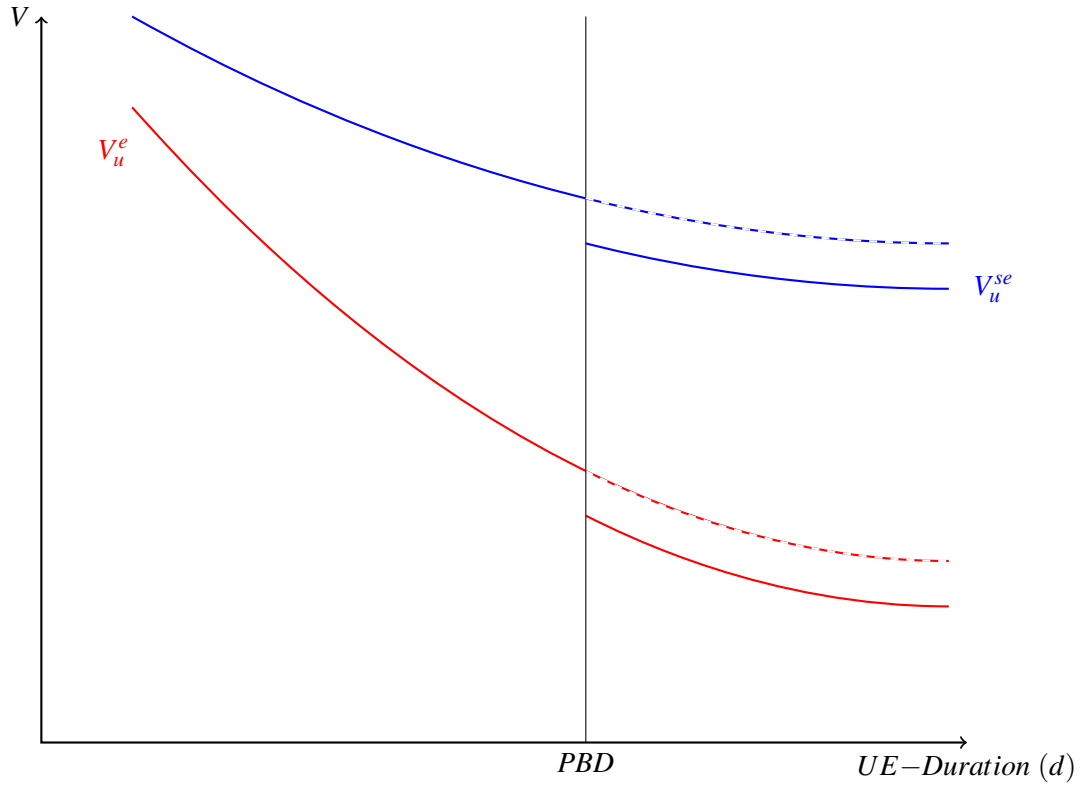
(b) Full-Time Equivalent Employment



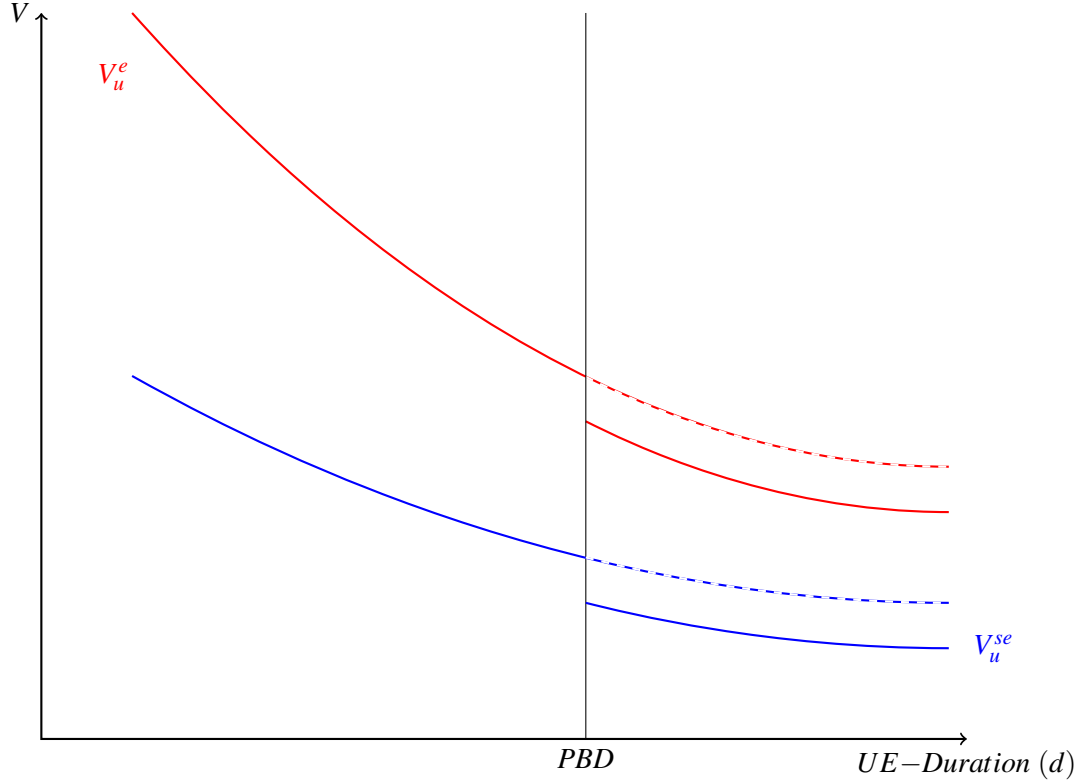
*Notes:* The Figure shows firm outcomes of non-team founders aged 35-65 (see the definition of our main estimation sample in [Table 1.2](#)) with previous unemployment spell in years after foundation split by self-reported motivation, i.e. *opportunity* vs. *pushed/necessity* driven entrepreneurship. We cover startups established between 2005 and 2011 from our linked dataset as described in [Section 1.2](#).

**Figure 1-6:** Selection into Self-Employment

Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with actual unemployment duration  $d$  according to the stylized model as explained in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration (PBD). At this point of unemployment duration the red/blue line drop by  $x = \bar{b} - \tilde{b}$  because UI benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  (compare [Equation \(1.8\)](#) and [Equation \(1.10\)](#)). In this example, the unemployed individual would first prefer to search for employment. But once the red line crosses the blue one: from this unemployment duration ( $d$ ) onward, the unemployed individual would prefer starting a business. Note that these results hold as long as depreciation in entrepreneurial skills is smaller in absolute terms than depreciation in employment skills and thus as long as the blue line has a less negative slope than the red line. If the value of becoming self-employed out of unemployment was independent of unemployment duration  $d$ , the blue line would be a horizontal line, and the associated pure selection channel (composition effect) could also explain our main results, i.e. that longer PBD leads to longer actual unemployment duration and more *pushed* startups.

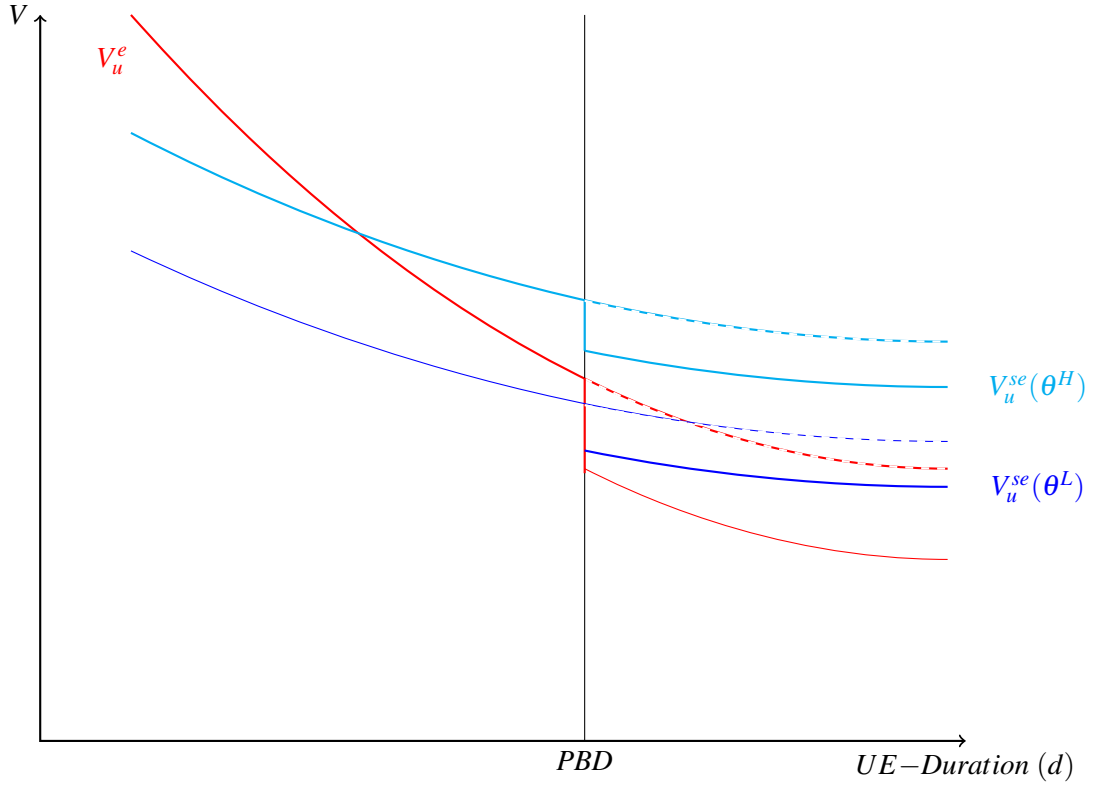
**Figure 1.7:** Selection into Self-Employment: High Entrepreneurial Ability

Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration according to the stylized model as explained in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration: at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as UI benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)). The unemployed individual learns to have such high entrepreneurial ability that she starts a business.

**Figure 1-8:** Selection into Employment: Low Entrepreneurial Ability

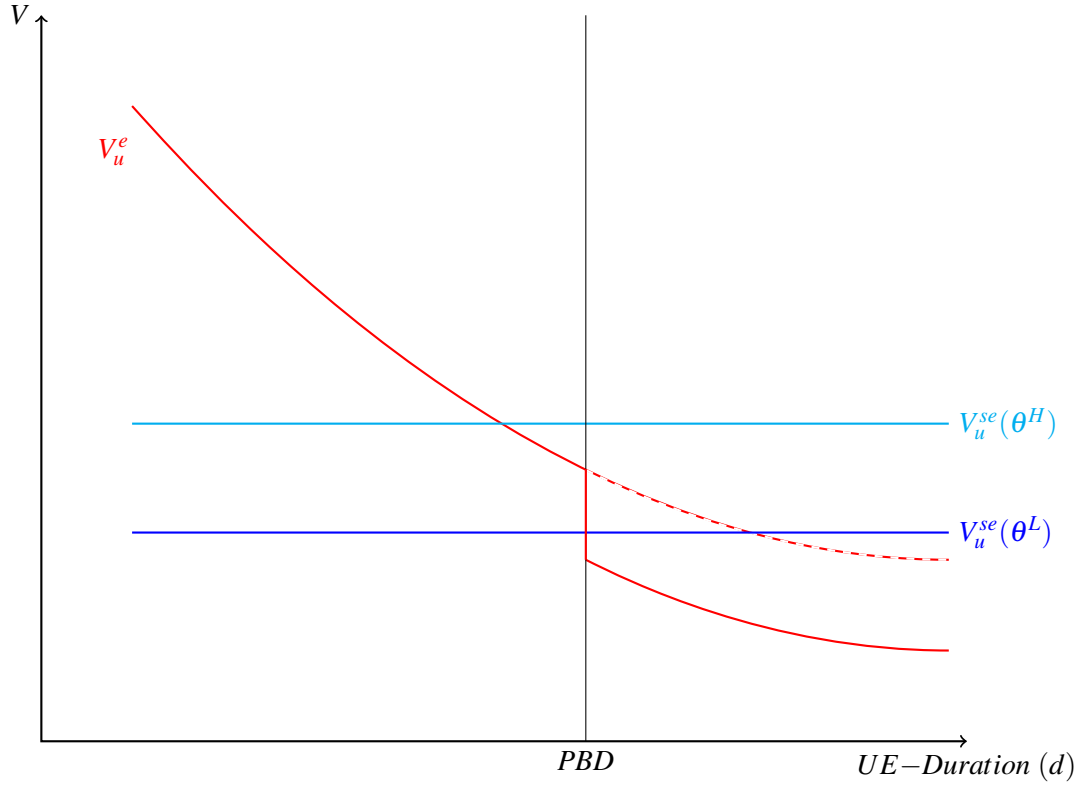
*Notes:* The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration according to the stylized model as explained in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration: at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as [UI](#) benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)). The unemployed individual learns to have such low entrepreneurial ability that she prefers employment.

**Figure 1.9:** PBD Rules can influence the Composition of Startups out of Unemployment



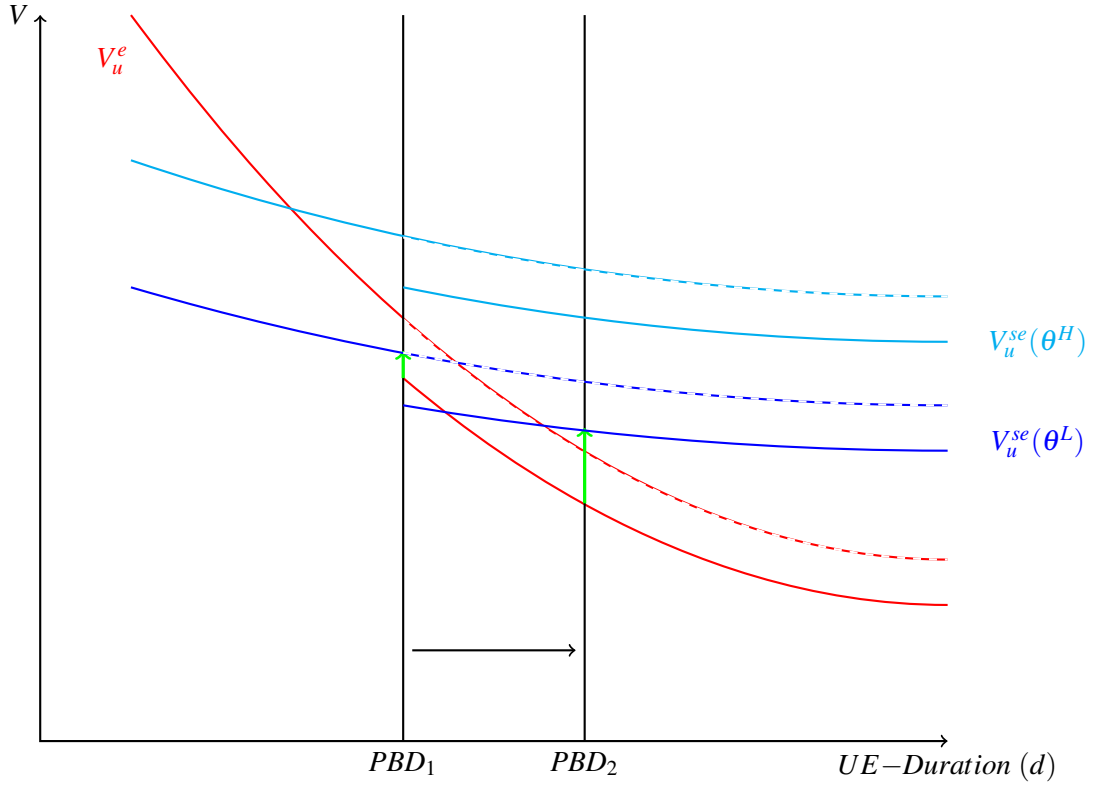
Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration according to the stylized model in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The cyan/blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration (PBD): at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as UI benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)). In this example, the unemployed individual with high ability  $\theta_H$  would decide to become self-employed after a short UI duration (cyan line), whereas the other unemployed individual  $\theta_L$  would intensify search for employment before reaching PBD (red line to the left of PBD), when  $V_{ut}^e$  suddenly drops below  $V_{ut}^{se}$  (blue line to the right of PBD). Here, the government could induce type  $H$  to become self-employed and  $L$  to search for employment.

**Figure 1·10:** If there was No Negative UI Duration Dependence concerning potential SE Outcomes



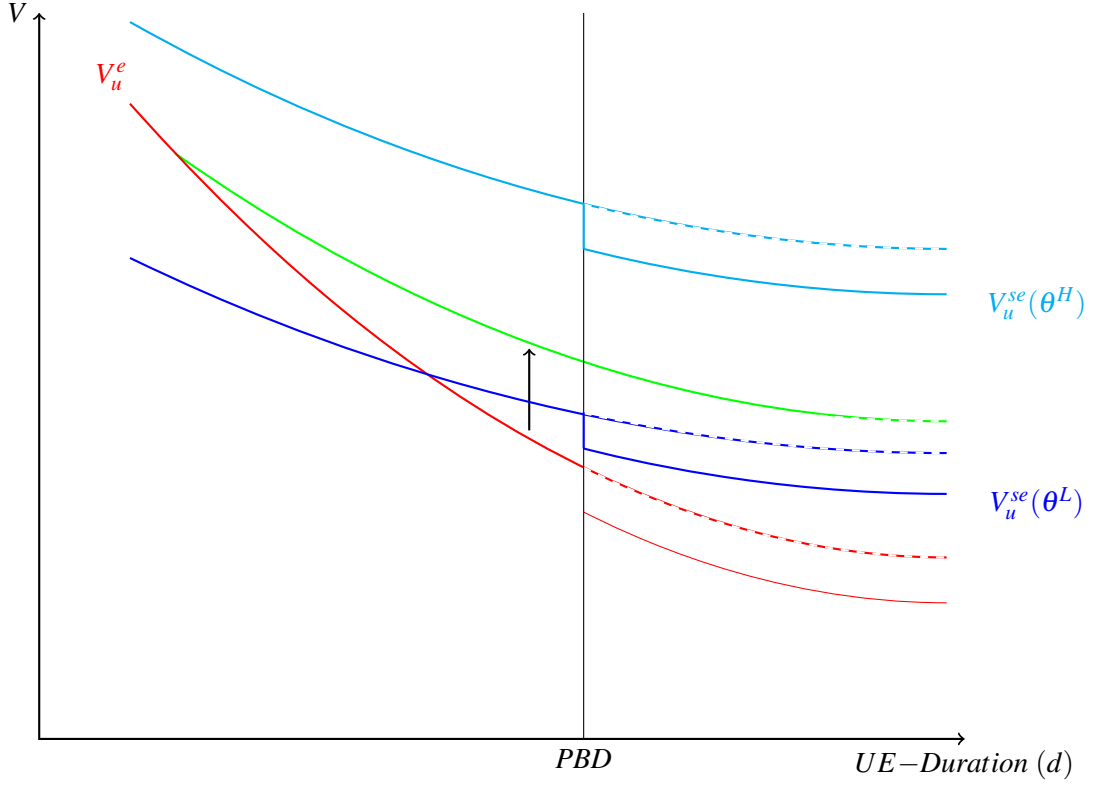
Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration  $d$  according to the stylized model as explained in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta = 0$ . The vertical black line marks **PBD**: at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as **UI** benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)).

**Figure 1-11:** PBD Rules can influence the Composition of Startups out of Unemployment (Increase in PBD)

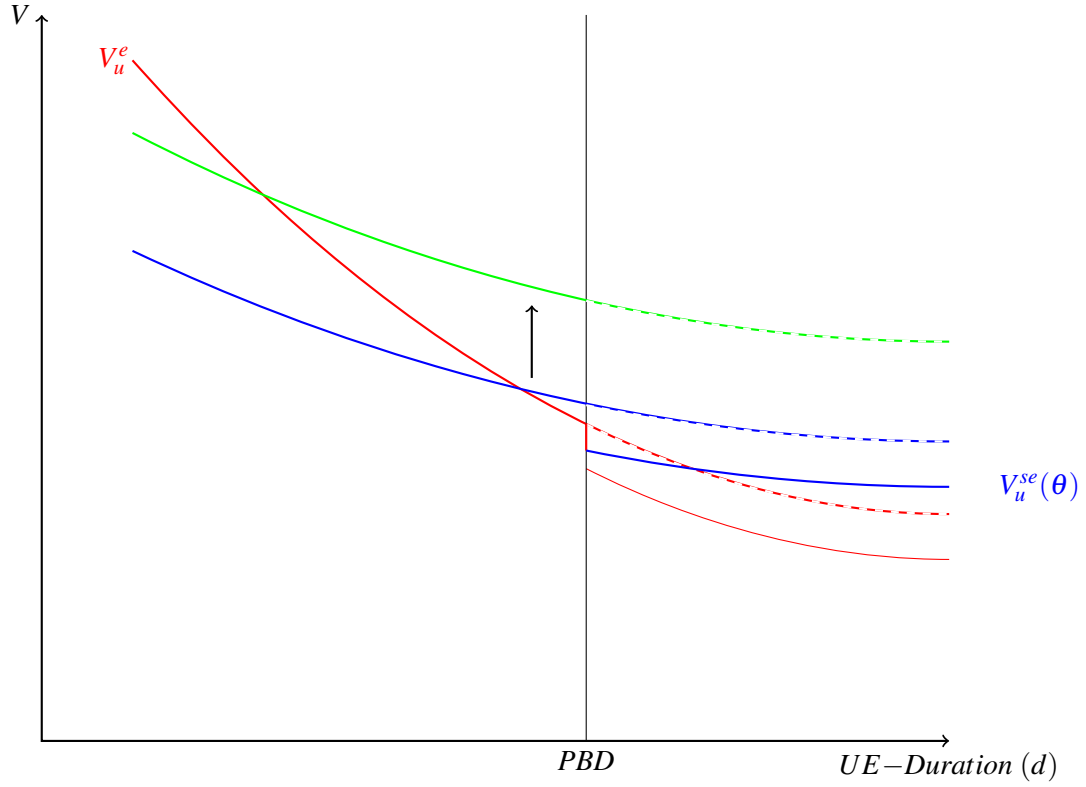


Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration according to the stylized model in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The cyan/blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration (PBD): at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as UI benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)). In this example, the government increases PBD (PBD moves to the right).  $PBD_1$  represents the initial potential benefit duration.  $PBD_2$  the extended one. At the initial  $PBD_1$ , the unemployed individual with high ability ( $\theta_H$ ) would decide to become self-employed after a short UI duration (cyan line), whereas the other unemployed individual ( $\theta_L$ ) would rather accept the next job when reaching  $PBD_1$  (red curve is above dark blue curve at  $PBD_1$ ). This illustrates, that in theory, increasing the potential benefit duration to  $PBD_2$  can change the composition among the unemployed individuals start up. Now, the value for becoming self-employed would be higher for both high individuals with  $\theta_H$  (opportunity entrepreneurs) and for individuals with  $\theta_L$  (necessity entrepreneurs) compared to the value for transitioning from unemployment to wage employment (at  $PBD_2$  the dark blue curve is now above the red curve). This illustrates how PBD can change the composition of startups created out of unemployment.



**Figure 1.12: Early Re-training for Wage-Employment**

Notes: The figure illustrates how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration  $d$  according to the stylized model in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The cyan/blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration (**PBD**): at this point of  $d$  the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as **UI** benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#), [Equation \(1.10\)](#)). By early retraining, the value function of searching for employment  $V_{ut}^e$  could be increased, as the green line indicates.

**Figure 1-13: Targeted Subsidies for Self-Employment**

Notes: The figure shows how the value functions for becoming employed  $V_{ut}^e$  and self-employed  $V_{ut}^{se}$  evolve with unemployment duration according to the stylized model in [Section 1.5](#). The red line depicts  $\frac{\partial V_{ut}^e}{\partial d} | \theta < 0$ . The blue line depicts  $\frac{\partial V_{ut}^{se}}{\partial d} | \theta$  for which it holds that:  $0 > \frac{\partial V_{ut}^{se}}{\partial d} | \theta > \frac{\partial V_{ut}^e}{\partial d} | \theta$ . The vertical black line marks the potential benefit duration (**PBD**): at this point of unemployment duration the red/blue line drops by  $x = \bar{b} - \tilde{b}$ , as **UI** benefits  $\bar{b}$  drop to the existential minimum  $\tilde{b}$  ([Equation \(1.8\)](#) and [Equation \(1.10\)](#)). By providing startup subsidies or special training for future self-employed, the government could increase  $V_{ut}^{se}$ , as shown by the green line.

## 1.8 Tables

**Table 1.1:** Definition of Necessity/Pushed vs. Opportunity Founders for Regression Sample

Motive to become entrepreneur	Opportunity entrepreneur	Pushed entrepreneur
Self-determined working	527	0
Realisation of business idea	255	0
Better earning potential	32	0
Tax incentives	3	0
No suitable employment options	0	169
Escape from unemployment	0	260
Forced by former employer	0	10
Total	817	439

*Notes:* This table is based on information from the [IAB/ZEW Start-Up Panel](#) and shows only our main regression sample. 1,300 non-team founders with maximal [UI](#) potential benefit duration that have been previously unemployed are considered in this table (see the definition of our main estimation sample in [Table 1.2](#)). Founders are asked about their motivation for starting a firm during the survey interview that is conducted when they enter the panel for the first time. Note that the intuition behind using the term *pushed* entrepreneur can be well understood by checking the spikes of the exit rate from unemployment into self-employment split by the motivation to start up, which is shown in [Figure I-1](#). This is corroborated when looking at [Table 1.2](#): Previously employed founders are much less likely to feel pushed into entrepreneurship (21% vs. 35% for previously unemployed founders.)

**Table 1.2:** Summary Statistics: Regression Sample - for previously Unemployed (above median unemployment duration) or Employed Founders

Variable	Regression sample of unemployed founders					Founders with > median UE duration					Previously not unemployed founders				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Unemployment (UE) Duration (months)	1291	4.79	4.56	0.03	36.17	641	8.02	4.52	3.12	36.17	0				
PBD (in months)	1291	12.32	4.25	0.59	37.42	641	13.31	4.62	3.52	37.42	0				
Tertiary degree (=1)	1291	0.28	0.45	0	1	641	0.30	0.46	0	1	1610	0.35	0.48	0	1
Founder: self-employed (SE) before (=1)	1291	0.15	0.36	0	1	641	0.16	0.37	0	1	1610	0.23	0.42	0	1
Managerial experience as employee (=1)	1291	0.13	0.33	0	1	641	0.14	0.34	0	1	1610	0.15	0.36	0	1
Female founder (=1)	1291	0.15	0.35	0	1	641	0.15	0.35	0	1	1610	0.13	0.34	0	1
Founder of non-German origin (=1)	1291	0.06	0.23	0	1	641	0.07	0.25	0	1	1610	0.05	0.21	0	1
SE Subsidy by Employment Agency (=1)	1291	0.75	0.43	0	1	641	0.73	0.44	0	1	1610	0.38	0.49	0	1
Industry Experience (in years)	1291	17.22	9.52	1.00	50.00	641	17.00	10.33	1.00	50.00	1610	16.56	9.15	1	54.00
Age of Founder (in years)	1291	44.44	5.93	35.09	65.11	641	45.28	6.20	35.34	65.11	1610	43.93	6.01	35	63.85
Sales in Year 1	1039	173,661	461,647	0	8,123,565	507	134,149	451,385	0	8,123,565	1309	399,872	2,467,627	0	84,370,000
Sales in Year 2	851	231,293	665,161	0	13,640,000	409	212,599	830,844	0	13,640,000	1067	400,055	1,121,272	0	24,180,000
FTE Employment after Year 1	1291	0.61	1.60	0	16.50	641	0.39	1.26	0	13.00	1610	1.02	3.28	0	74.50
FTE Employment after Year 2	1272	0.85	2.08	0	28.25	628	0.54	1.44	0	12.50	1597	1.45	4.26	0	95.75
Pushed/Necessity Motive (=1)	1256	0.35	0.48	0	1	631	0.39	0.49	0	1	1531	0.21	0.41	0	1
Technology-intensive services	1291	0.19	0.39	0	1	641	0.20	0.40	0	1	1610	0.23	0.42	0	1
High-technology manufacturing	1291	0.09	0.28	0	1	641	0.08	0.28	0	1	1610	0.12	0.33	0	1
Skill-intensive services	1291	0.05	0.21	0	1	641	0.05	0.22	0	1	1610	0.08	0.27	0	1
Software supply and consultancy	1291	0.03	0.18	0	1	641	0.03	0.18	0	1	1610	0.06	0.23	0	1
Non-high-tech manufacturing	1291	0.12	0.33	0	1	641	0.11	0.31	0	1	1610	0.12	0.33	0	1
Other business-oriented services	1291	0.07	0.25	0	1	641	0.07	0.25	0	1	1610	0.05	0.22	0	1
Cons.-or. services in creative sect.	1291	0.02	0.15	0	1	641	0.02	0.15	0	1	1610	0.03	0.16	0	1
Consumer-oriented services	1291	0.10	0.30	0	1	641	0.10	0.30	0	1	1610	0.07	0.25	0	1
Construction	1291	0.16	0.36	0	1	641	0.15	0.35	0	1	1610	0.11	0.31	0	1
Retail & wholesale	1291	0.18	0.38	0	1	641	0.19	0.39	0	1	1610	0.14	0.35	0	1

**Notes:** This table shows summary statistics for non-team founders that have started their business out of unemployment (first panel). In the second panel, our table shows the same statistics for the sub-sample of these non-team founders that had equal or greater than median unemployment duration before starting up. The table only includes those individuals that have maximum PBD at the beginning of the unemployment spell, since the empirical strategies require that our main regression sample consists of non-team founders that have achieved these criteria. Note that out of the around 4,000 non-team founders having unemployment experience before starting up in our data, approximately 1,300 satisfy the criteria to be included in our main regression sample: they became unemployed between 2003 and 2011, were between 35 and 65 years old when becoming unemployed, are eligible to maximum potential benefit duration, and have information on all included control variables available. Finally, the the right-hand panel of this table shows the same summary statistics for a reference group of founders who have started their business out of employment, i.e. they have not been previously unemployed.

**Table 1.3:** OLS Results: Actual Benefit Duration (ABD) on Motivation of Founder and Firm Outcomes

	(1) Necessity Motive (=1)	(2) Sales Year 1 (log)	(3) Sales Year 2 (log)	(4) FTE Employment Year 1 (log)	(5) FTE Employment Year 2 (log)
<b>unemployment (UE) Duration (in months)</b>	<b>0.017***</b> (0.003)	<b>-0.139***</b> (0.027)	<b>-0.096***</b> (0.018)	<b>-0.016***</b> (0.003)	<b>-0.023***</b> (0.003)
Tertiary degree (=1)	-0.050 (0.032)	-0.663** (0.283)	-0.029 (0.168)	0.076** (0.034)	0.074* (0.039)
Founder was SE before (=1)	-0.009 (0.037)	0.031 (0.336)	-0.125 (0.211)	-0.006 (0.040)	-0.018 (0.045)
Managerial Experience as Employee (=1)	-0.059 (0.038)	0.169 (0.350)	0.618*** (0.164)	0.135*** (0.049)	0.173*** (0.056)
Industry Experience (in years)	0.003* (0.001)	0.034** (0.014)	0.003 (0.009)	0.001 (0.001)	0.001 (0.002)
Female founder (=1)	0.013 (0.039)	-0.993** (0.404)	-0.361* (0.210)	0.069 (0.046)	0.104** (0.052)
Founder of non-German origin (=1)	0.033 (0.060)	-1.758*** (0.673)	-0.509 (0.418)	-0.015 (0.055)	-0.048 (0.054)
SE Subsidy by Federal Employment Agency (=1)	0.073** (0.032)	-0.413 (0.280)	-0.289* (0.168)	-0.056* (0.033)	-0.086** (0.037)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	1256	1039	851	1291	1272
R-sq.	0.063	0.122	0.126	0.150	0.158
Mean of dependent variable (abs. value for log-terms)	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the OLS regression of our main outcome variables (motivation for starting up; sales and employment growth after year 1, 2) on the founders' actual unemployment benefit duration (ABD) before starting up. We control for the founders' education, their previous work experience, and individual characteristics. Moreover, we include year and industry (of the startup) fixed effects. We also include dummies to control for the receipt of subsidies from the Federal Employment Agency and for funding by the KfW bank (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, who were 35 to 65 years old when becoming unemployed, and for whom information on all included control variables is available.

**Table 1.4:** OLS Results: ABD on Motivation of Founder and Firm Outcomes focusing on Non-Manufacturing Sector

	(1) Necessity Motive (=1)	(2) Sales Year 1 (log)	(3) Sales Year 2 (log)	(4) FTE Employment Year 1 (log)	(5) FTE Employment Year 2 (log)
<b>UE Duration (in months)</b>	<b>0.018***</b> (0.003)	<b>-0.128***</b> (0.029)	<b>-0.102***</b> (0.021)	<b>-0.016***</b> (0.003)	<b>-0.024***</b> (0.003)
Tertiary degree (=1)	-0.069* (0.036)	-0.392 (0.290)	0.143 (0.168)	0.059* (0.035)	0.057 (0.041)
Founder was SE before (=1)	0.009 (0.042)	0.119 (0.343)	-0.262 (0.230)	0.015 (0.045)	0.031 (0.051)
Managerial Experience as Employee (=1)	-0.096** (0.042)	0.022 (0.381)	0.457** (0.184)	0.130** (0.053)	0.178*** (0.060)
Industry Experience (in years)	0.002 (0.002)	0.021 (0.015)	0.001 (0.011)	0.002 (0.002)	0.002 (0.002)
Female founder (=1)	0.006 (0.043)	-0.693* (0.413)	-0.512** (0.232)	0.077 (0.051)	0.096* (0.056)
Founder of non-German origin (=1)	-0.047 (0.064)	-1.820** (0.724)	-0.843* (0.496)	-0.076 (0.047)	-0.080* (0.047)
SE Subsidy by Federal Employment Agency (=1)	0.070* (0.036)	-0.629** (0.309)	-0.277 (0.202)	-0.054 (0.035)	-0.069* (0.040)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	999	815	661	1022	1009
R-sq.	0.076	0.103	0.145	0.167	0.168
Mean of dependent variable (abs. value for log-terms)	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the OLS regression of our main outcome variables (motivation for starting up; sales and employment growth after year 1, 2) on the founders' actual unemployment benefit duration (ABD) before starting up in the non-manufacturing sector (75% of our sample). We control for the founders' education, their previous work experience, and individual characteristics. Moreover, we include year and industry (of the startup) fixed effects. We also include dummies to control for the receipt of subsidies from the Federal Employment Agency and for funding by the KfW bank (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, who were 35 to 65 years old when becoming unemployed, and for whom information on all included control variables is available.

**Table 1.5:** Potential UI Benefit Duration (in months) based on Contributions/Age

(1) Contribution Months	(2) before 02/2006	(3) Age Rules	(4) from 02/2006 until 12/2007	(5) Age Rules	(6) since 01/2008	(7) Age Rules
12	6		6		6	
18	9		9		9	
24	12		12		12	
30	15	≥ 45	15	≥ 55	15	≥ 50
36	18		18	≥ 55	18	≥ 55
44	22	≥ 47	18	≥ 55	22	≥ 58
48	24		18	≥ 55	24	≥ 58
52	26	≥ 52	18	≥ 55	24	≥ 58
64	32	≥ 57	18	≥ 55	24	≥ 58

*Notes:* The table shows how potential unemployment insurance (UI) benefit duration (PBD) varies with the number of contribution months (column 1), i.e. the number of months a worker paid UI contributions that are mandatory for jobs covered by the social security. The rules state that after having satisfied the minimum eligibility requirement (e.g. at least 12 contributions within last 24 months) half of the number of contribution months translate into PBD. However, at some point a maximum PBD is reached and additional contribution months can no longer increase PBD. This table presents the age rules for maximum PBD, i.e. for which age groups the indicated PBD is available, since only with increasing age does the maximum PBD increase. Maximum PBD by age group is also shown in Table 1.6. Columns (2) and (3) show the PBD regime before February 2006, columns (4) and (5) between February 2006 and December 2007 and columns (6) and (7) since January 2008.

**Table 1.6:** Maximum Potential UI Benefit Duration (in months) in Germany

(1) Age	(2) before 02/2006	(3) Reduction in months	(4) from 02/2006 until 12/2007	(5) Extension in months	(6) since 01/2008	(7) Net-Effect in months
<45	12	0	12	0	12	0
45-46	18	-6	12	0	12	-6
47-49	22	-10	12	0	12	-10
50-51	22	-10	12	+3	15	-7
52-54	26	-14	12	+3	15	-11
55-56	26	-8	18	0	18	-8
57	32	-14	18	0	18	-14
>58	32	-14	18	+6	24	-8

*Notes:* The table shows how potential unemployment insurance (UI) benefit duration (PBD) varies by age group and over time for unemployed individuals who had worked for at least the number of contribution months within the last five years (seven years before 02/2006) necessary to get the maximum PBD of their age group according to Table 1.5 without intermittent UI spell. This table shows that the reform of February 2006 represents a considerable decline in PBD for workers aged above 45 years. In contrast, the reform of January 2008 partially increased PBD again. However, in total the net effect across both reforms demonstrates that all age groups beyond 45 years suffered a considerable decline in PBD (cf. Section 1.3).

**Table 1.7:** OLS Results: Potential Benefit Duration (PBD) on Actual Benefit Duration (ABD), Motivation of Founder and Firm Outcomes

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>PBD (in months)</b>	<b>0.471***</b> (0.048)	<b>0.023***</b> (0.003)	<b>-0.036</b> (0.024)	<b>-0.049**</b> (0.022)	<b>-0.004</b> (0.004)	<b>-0.009**</b> (0.004)
Tertiary degree (=1)	-0.386 (0.291)	-0.073** (0.032)	-0.636** (0.289)	0.027 (0.173)	0.079** (0.034)	0.081** (0.040)
Founder was SE before (=1)	0.116 (0.338)	-0.014 (0.037)	-0.001 (0.341)	-0.095 (0.213)	-0.009 (0.040)	-0.019 (0.045)
Managerial Experience as Employee (=1)	-0.004 (0.346)	-0.069* (0.038)	0.160 (0.356)	0.609*** (0.168)	0.134*** (0.050)	0.173*** (0.057)
Industry Experience (in years)	-0.013 (0.015)	0.001 (0.001)	0.036** (0.014)	0.007 (0.009)	0.001 (0.002)	0.002 (0.002)
Female founder (=1)	0.176 (0.326)	0.013 (0.039)	-1.075*** (0.405)	-0.369* (0.221)	0.065 (0.047)	0.100* (0.053)
Founder of non-German origin (=1)	0.299 (0.500)	0.034 (0.059)	-1.853*** (0.674)	-0.553 (0.446)	-0.021 (0.056)	-0.056 (0.057)
SE Subsidy by Federal Employment Agency (=1)	-0.301 (0.292)	0.061* (0.031)	-0.401 (0.281)	-0.277 (0.171)	-0.053 (0.034)	-0.080** (0.038)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1291	1256	1039	851	1291	1272
R-sq.	0.256	0.077	0.099	0.096	0.133	0.134
Mean of dependent variable (abs. value for log-terms)	4.785	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the OLS regression of our main outcome variables (ABD, motivation for starting up; sales and employment growth after year 1, 2) on the founders' potential benefit duration (ABD) before starting up. We control for the founders' education, their previous work experience, and individual characteristics. Moreover, we include year and industry (of the startup) fixed effects. We also include dummies to control for the receipt of subsidies from the Federal Employment Agency and for funding by the KfW bank (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, who were 35 to 65 years old when becoming unemployed, and for whom information on all included control variables is available.



**Table 1.8:** OLS Results: PBD on ABD, Motivation of Founder and Firm Outcomes for Non-Manufacturing Sector

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>PBD (in months)</b>	<b>0.484***</b> (0.053)	<b>0.022***</b> (0.004)	<b>-0.067***</b> (0.026)	<b>-0.070***</b> (0.026)	<b>-0.010***</b> (0.003)	<b>-0.015***</b> (0.004)
Tertiary degree (=1)	-0.462 (0.335)	-0.092*** (0.036)	-0.324 (0.295)	0.225 (0.168)	0.069* (0.036)	0.072* (0.041)
Founder was SE before (=1)	0.443 (0.369)	0.015 (0.042)	0.065 (0.351)	-0.250 (0.235)	0.008 (0.046)	0.023 (0.052)
Managerial Experience as Employee (=1)	0.052 (0.387)	-0.098** (0.043)	0.005 (0.383)	0.430** (0.188)	0.130** (0.053)	0.178*** (0.061)
Industry Experience (in years)	0.001 (0.016)	0.001 (0.002)	0.022 (0.015)	0.005 (0.011)	0.002 (0.002)	0.003 (0.002)
Female founder (=1)	0.329 (0.370)	0.007 (0.043)	-0.787* (0.415)	-0.539** (0.244)	0.072 (0.052)	0.090 (0.057)
Founder of non-German origin (=1)	0.303 (0.546)	-0.040 (0.065)	-1.882*** (0.723)	-0.876 (0.533)	-0.081* (0.047)	-0.088* (0.049)
SE Subsidy by Federal Employment Agency (=1)	-0.287 (0.342)	0.062* (0.036)	-0.597* (0.310)	-0.252 (0.202)	-0.049 (0.036)	-0.060 (0.041)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1022	999	815	661	1022	1009
R-sq.	0.266	0.082	0.085	0.116	0.153	0.145
Mean of dependent variable (abs. value for log-terms)	4.895	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the OLS regression of our main outcome variables (ABD, motivation for starting up; sales and employment growth after year 1, 2) on the founders' potential benefit duration (PBD) before starting up in the non-manufacturing sector (75% of our sample). We control for the founders' education, their previous work experience, and individual characteristics. Moreover, we include year and industry (of the startup) fixed effects. We also include dummies to control for the receipt of subsidies from the Federal Employment Agency and for funding by the KfW bank (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, who were 35 to 65 years old when becoming unemployed, and for whom information on all included control variables is available.

**Table 1.9:** IV Results for Reform 2006: Potential Benefit Duration on Actual Benefit Duration (ABD), Motivation of Founder and Firm Outcomes

	(1) PBD (in months)	(2) UE Duration (in months)	(3) Necessity Motive (=1)	(4) Sales Year 1 (log)	(5) Sales Year 2 (log)	(6) FTE Employment Year 1 (log)	(7) FTE Employment Year 2 (log)
<b>PBD (in months)</b>		<b>0.661***</b> (0.094)	<b>0.015**</b> (0.007)	<b>0.034</b> (0.052)	<b>-0.072*</b> (0.039)	<b>0.003</b> (0.007)	<b>-0.006</b> (0.008)
Tertiary degree (=1)	0.313 (0.221)	-0.513* (0.289)	-0.075** (0.032)	-0.689** (0.289)	0.048 (0.169)	0.071** (0.034)	0.078** (0.039)
Founder was SE before (=1)	-0.025 (0.278)	0.121 (0.345)	-0.022 (0.037)	-0.014 (0.339)	-0.093 (0.210)	-0.012 (0.039)	-0.020 (0.045)
Managerial Experience as Employee (=1)	0.301 (0.305)	-0.023 (0.348)	-0.077** (0.038)	0.144 (0.353)	0.613*** (0.165)	0.130*** (0.050)	0.171*** (0.057)
Industry Experience (in years)	0.014 (0.011)	-0.017 (0.014)	0.001 (0.001)	0.033** (0.015)	0.009 (0.010)	0.001 (0.002)	0.001 (0.002)
Female founder (=1)	0.002 (0.274)	0.198 (0.325)	0.004 (0.038)	-1.088*** (0.402)	-0.366* (0.216)	0.063 (0.046)	0.099* (0.052)
Founder of non-German origin (=1)	0.021 (0.372)	0.207 (0.505)	0.042 (0.058)	-1.897*** (0.666)	-0.530 (0.427)	-0.023 (0.055)	-0.057 (0.056)
SE Subsidy by Federal Employment Agency (=1)	0.398* (0.214)	-0.406 (0.294)	0.067** (0.031)	-0.437 (0.279)	-0.269 (0.167)	-0.056* (0.033)	-0.081** (0.038)
<b>IV_06</b>	<b>-8.743***</b> (0.505)						
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		299.421	286.66	223.603	220.761	299.421	296.11
N	1291	1291	1256	1039	851	1291	1272
R-sq.	0.470	0.234	0.083	0.094	0.094	0.130	0.134
Mean of dependent variable (abs. value for log-terms)	12.324	4.785	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the IV regression of our outcome variables (ABD, motivation for starting up; sales/employment growth after year 1, 2) on the founders' PBD before starting up that is instrumented by IV06 (Section 1.3.2). Column 1 shows the first-stage regression of the IV model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.10: IV Results for Reform 2006: PBD on ABD, Motivation of Founder, Firm Outcomes for Non-Manufacturing Sector**

	(1) PBD (in months)	(2) UE Duration (in months)	(3) Necessity Motive (=1)	(4) Sales Year 1 (log)	(5) Sales Year 2 (log)	(6) FTE Employment Year 1 (log)	(7) FTE Employment Year 2 (log)
<b>PBD (in months)</b>		<b>0.722**</b> (0.109)	<b>0.010</b> (0.008)	<b>0.004</b> (0.056)	<b>-0.115**</b> (0.051)	<b>-0.008</b> (0.006)	<b>-0.018**</b> (0.008)
Tertiary degree (=1)	0.434* (0.248)	-0.641* (0.331)	-0.089** (0.036)	-0.390 (0.296)	0.269 (0.167)	0.064* (0.036)	0.071* (0.041)
Founder was SE before (=1)	-0.296 (0.312)	0.517 (0.385)	0.006 (0.042)	0.078 (0.350)	-0.270 (0.234)	0.005 (0.044)	0.019 (0.051)
Managerial Experience as Employee (=1)	0.137 (0.350)	0.068 (0.387)	-0.105** (0.042)	-0.000 (0.377)	0.423** (0.186)	0.127** (0.053)	0.176*** (0.060)
Industry Experience (in years)	0.011 (0.013)	-0.006 (0.016)	0.001 (0.002)	0.019 (0.016)	0.008 (0.012)	0.002 (0.002)	0.002 (0.002)
Female founder (=1)	0.086 (0.313)	0.312 (0.368)	0.001 (0.042)	-0.818** (0.412)	-0.533** (0.238)	0.069 (0.051)	0.088 (0.056)
Founder of non-German origin (=1)	-0.101 (0.395)	0.252 (0.554)	-0.032 (0.063)	-1.922*** (0.708)	-0.828 (0.508)	-0.079* (0.046)	-0.085* (0.048)
SE Subsidy by Federal Employment Agency (=1)	0.333 (0.234)	-0.389 (0.341)	0.071** (0.036)	-0.633** (0.307)	-0.236 (0.198)	-0.049 (0.035)	-0.058 (0.040)
<b>IV_06</b>	<b>-8.401***</b> (0.569)						
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		217.975	209.815	155.049	153.108	217.975	216.04
N	1022	1022	999	815	661	1022	1009
R-sq.	0.477	0.232	0.083	0.079	0.110	0.153	0.145
Mean of dependent variable (abs. value for log-terms)	12.327	4.895	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the **IV** regression of our outcome variables (**ABD**, motivation for starting up; sales/employment growth after year 1, 2) on the founders' **PBD** before starting up in the non-manufacturing sector (75% of the sample) that is instrumented by **IV06** (Section 1.3.2). Column 1 shows the first-stage regression of the **IV** model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.11:** IV Results for Reform 2006: Actual Benefit Duration (ABD) on Motivation of Founder and Firm Outcomes

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>UE Duration (in months)</b>		<b>0.022**</b> (0.010)	<b>0.060</b> (0.095)	<b>-0.118*</b> (0.063)	<b>0.005</b> (0.011)	<b>-0.010</b> (0.012)
Tertiary degree (=1)	-0.313 (0.311)	-0.063** (0.031)	-0.662** (0.287)	-0.030 (0.164)	0.074** (0.034)	0.073* (0.039)
Founder was SE before (=1)	0.067 (0.357)	-0.024 (0.037)	-0.023 (0.343)	-0.129 (0.208)	-0.013 (0.040)	-0.020 (0.045)
Managerial Experience as Employee (=1)	0.144 (0.380)	-0.076** (0.038)	0.146 (0.358)	0.624*** (0.162)	0.130*** (0.050)	0.171*** (0.057)
Industry Experience (in years)	-0.008 (0.015)	0.001 (0.001)	0.035** (0.015)	0.003 (0.010)	0.001 (0.002)	0.001 (0.002)
Female founder (=1)	0.240 (0.361)	-0.001 (0.038)	-1.118*** (0.408)	-0.357* (0.205)	0.062 (0.047)	0.101* (0.052)
Founder of non-German origin (=1)	0.226 (0.505)	0.037 (0.059)	-1.928*** (0.670)	-0.489 (0.400)	-0.024 (0.055)	-0.054 (0.054)
SE Subsidy by Federal Employment Agency (=1)	-0.086 (0.311)	0.075** (0.031)	-0.423 (0.281)	-0.288* (0.166)	-0.054 (0.034)	-0.085** (0.037)
<b>IV_06</b>	<b>-5.815***</b> (0.921)					
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		39.831	23.487	24.016	40.992	41.07
N	1256	1256	1039	851	1291	1272
R-sq.	0.189	0.075	0.072	0.124	0.120	0.149
Mean of dependent variable (abs. value for log-terms)	4.828	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the IV regression of our outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' actual benefit duration (ABD) before starting up that is instrumented by IV06 (Section 1.3.2). Column 1 shows the first-stage regression of the IV model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.12:** IV Results for Reform 2006: ABD on Motivation of Founder, Firm Outcomes for Non-Manufacturing Sector

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log) (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>UE Duration (in months)</b>		<b>0.014</b> (0.011)	<b>0.007</b> (0.092)	<b>-0.171**</b> (0.077)	<b>-0.011</b> (0.009)	<b>-0.025**</b> (0.011)
Tertiary degree (=1)	-0.305 (0.357)	-0.081** (0.035)	-0.387 (0.289)	0.145 (0.163)	0.057 (0.035)	0.056 (0.040)
Founder was SE before (=1)	0.243 (0.374)	-0.001 (0.042)	0.074 (0.346)	-0.283 (0.228)	0.011 (0.044)	0.030 (0.050)
Managerial Experience as Employee (=1)	0.158 (0.434)	-0.105** (0.042)	-0.001 (0.380)	0.475*** (0.183)	0.128** (0.053)	0.177*** (0.060)
Industry Experience (in years)	0.002 (0.017)	0.001 (0.002)	0.019 (0.016)	0.002 (0.012)	0.001 (0.002)	0.002 (0.002)
Female founder (=1)	0.356 (0.407)	-0.003 (0.042)	-0.823** (0.419)	-0.485** (0.227)	0.072 (0.051)	0.095* (0.055)
Founder of non-German origin (=1)	0.198 (0.551)	-0.035 (0.063)	-1.925*** (0.709)	-0.781* (0.464)	-0.076* (0.046)	-0.078* (0.047)
SE Subsidy by Federal Employment Agency (=1)	-0.084 (0.367)	0.075** (0.035)	-0.631** (0.306)	-0.280 (0.201)	-0.053 (0.035)	-0.068* (0.039)
<b>IV_06</b>	<b>-5.987***</b> (1.047)					
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		32.673	19.62	19.994	34.939	34.852
N	999	999	815	661	1022	1009
R-sq.	0.209	0.089	0.077	0.124	0.165	0.168
Mean of dependent variable (abs. value for log-terms)	4.926	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the **IV** regression of our outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' actual benefit duration (**ABD**) before starting up in the non-manufacturing sector (75% of the sample) that is instrumented by *IV06* (Section 1.3.2). Column 1 shows the first-stage regression of the **IV** model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.13: IV Results for Reforms 2006 & 2008: PBD on Actual Benefit Duration (ABD), Motivation of Founder and Firm Outcomes**

	(1) PBD (in months)	(2) UE Duration (in months)	(3) Necessity Motive (=1)	(4) Sales Year 1 (log)	(5) Sales Year 2 (log)	(6) FTE Employment Year 1 (log)	(7) FTE Employment Year 2 (log)
<b>PBD (in months)</b>		<b>0.649***</b> (0.093)	<b>0.015**</b> (0.007)	<b>0.030</b> (0.049)	<b>-0.074**</b> (0.038)	<b>0.004</b> (0.007)	<b>-0.006</b> (0.008)
Tertiary degree (=1)	0.247 (0.217)	-0.467 (0.286)	-0.076** (0.032)	-0.671** (0.288)	0.057 (0.168)	0.075** (0.034)	0.080** (0.040)
Founder was SE before (=1)	-0.157 (0.268)	0.185 (0.342)	-0.025 (0.037)	0.012 (0.338)	-0.082 (0.207)	-0.006 (0.039)	-0.018 (0.045)
Managerial Experience as Employee (=1)	0.291 (0.302)	-0.006 (0.346)	-0.077** (0.038)	0.150 (0.353)	0.615*** (0.165)	0.131*** (0.050)	0.172*** (0.057)
Industry Experience (in years)	0.008 (0.011)	-0.014 (0.014)	0.000 (0.001)	0.034** (0.015)	0.009 (0.010)	0.001 (0.002)	0.001 (0.002)
Female founder (=1)	-0.015 (0.273)	0.186 (0.321)	0.005 (0.038)	-1.100*** (0.401)	-0.368* (0.216)	0.062 (0.047)	0.099* (0.052)
Founder of non-German origin (=1)	0.134 (0.364)	0.219 (0.504)	0.042 (0.058)	-1.899*** (0.666)	-0.535 (0.427)	-0.022 (0.054)	-0.057 (0.056)
SE Subsidy by Federal Employment Agency (=1)	0.446** (0.210)	-0.411 (0.295)	0.067** (0.031)	-0.446 (0.280)	-0.272 (0.167)	-0.058* (0.033)	-0.081** (0.038)
<b>IV_06</b>	<b>-9.316***</b> (0.514)						
<b>IV_08</b>	<b>2.356***</b> (0.353)						
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		41.07	147.61	115.608	119.869	154.434	152.383
N	1291	1291	1256	1039	851	1291	1272
R-sq.	0.487	0.242	0.084	0.096	0.094	0.133	0.135
Mean of dependent variable (abs. value for log-terms)	12.324	4.785	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the IV regression of our outcome variables (ABD, motivation for starting up; sales/employment growth after year 1, 2) on the founders' PBD before starting up that is instrumented by IV06 and IV08 (Section 1.3.2). Column 1 shows the first-stage regression of the IV model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.14:** IV Results for Reforms 2006 & 2008: PBD on ABD, Motiv. of Founder, Firm Outcomes focusing on Non-Manufacturing Sector

	(1) PBD (in months)	(2) UE Duration (in months)	(3) Necessity Motive (=1)	(4) Sales Year 1 (log)	(5) Sales Year 2 (log)	(6) FTE Employment Year 1 (log)	(7) FTE Employment Year 2 (log)
<b>PBD (in months)</b>		<b>0.721**</b> (0.108)	<b>0.010</b> (0.008)	<b>-0.002</b> (0.055)	<b>-0.115**</b> (0.050)	<b>-0.007</b> (0.007)	<b>-0.017**</b> (0.008)
Tertiary degree (=1)	0.391 (0.244)	-0.585* (0.327)	-0.091** (0.036)	-0.371 (0.295)	0.266 (0.166)	0.066* (0.036)	0.072* (0.041)
Founder was SE before (=1)	-0.396 (0.304)	0.595 (0.380)	0.003 (0.042)	0.097 (0.352)	-0.275 (0.233)	0.009 (0.044)	0.021 (0.051)
Managerial Experience as Employee (=1)	0.112 (0.348)	0.096 (0.381)	-0.106** (0.043)	0.006 (0.378)	0.422** (0.185)	0.128** (0.053)	0.177*** (0.060)
Industry Experience (in years)	0.006 (0.012)	-0.003 (0.016)	0.001 (0.002)	0.020 (0.016)	0.008 (0.012)	0.002 (0.002)	0.002 (0.002)
Female founder (=1)	0.065 (0.312)	0.298 (0.362)	0.002 (0.042)	-0.825** (0.411)	-0.533** (0.238)	0.068 (0.051)	0.088 (0.056)
Founder of non-German origin (=1)	0.031 (0.386)	0.214 (0.558)	-0.030 (0.063)	-1.928*** (0.710)	-0.824 (0.510)	-0.081* (0.046)	-0.086* (0.048)
SE Subsidy by Federal Employment Agency (=1)	0.378 (0.231)	-0.409 (0.342)	0.071** (0.036)	-0.636** (0.306)	-0.234 (0.197)	-0.050 (0.035)	-0.058 (0.040)
<b>IV_06</b>	<b>-8.955***</b> (0.579)						
<b>IV_08</b>	<b>2.115***</b> (0.385)						
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		34.852	107.841	78.456	84.474	112.178	110.896
N	1022	1022	999	815	661	1022	1009
R-sq.	0.493	0.241	0.084	0.080	0.110	0.155	0.146
Mean of dependent variable (abs. value for log-terms)	12.327	4.895	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the **IV** regression of our outcome variables (**ABD**, motivation for starting up; sales/employment growth after year 1, 2) on the founders' **PBD** before starting up in the non-manufacturing sector (75% of the sample) that is instrumented by *IV06* and *IV08* (Section 1.3.2). Column 1 shows the first-stage regression of the **IV** model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.15:** IV Results for Reforms 2006 & 2008: Actual Benefit Duration (ABD) on Motivation of Founder and Firm Outcomes

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>UE Duration (in months)</b>		<b>0.023**</b> (0.010)	<b>0.058</b> (0.095)	<b>-0.121*</b> (0.063)	<b>0.004</b> (0.011)	<b>-0.010</b> (0.012)
Tertiary degree (=1)	-0.314 (0.311)	-0.065** (0.031)	-0.649** (0.287)	-0.019 (0.163)	0.077** (0.034)	0.075* (0.039)
Founder was SE before (=1)	0.065 (0.358)	-0.029 (0.037)	-0.002 (0.342)	-0.111 (0.205)	-0.007 (0.040)	-0.016 (0.045)
Managerial Experience as Employee (=1)	0.143 (0.381)	-0.077** (0.038)	0.150 (0.358)	0.627*** (0.161)	0.131*** (0.050)	0.172*** (0.056)
Industry Experience (in years)	-0.008 (0.015)	0.001 (0.001)	0.036** (0.015)	0.004 (0.010)	0.001 (0.002)	0.001 (0.002)
Female founder (=1)	0.242 (0.361)	-0.001 (0.038)	-1.126*** (0.407)	-0.361* (0.204)	0.061 (0.047)	0.100* (0.052)
Founder of non-German origin (=1)	0.222 (0.505)	0.036 (0.058)	-1.930*** (0.670)	-0.497 (0.397)	-0.023 (0.055)	-0.053 (0.054)
SE Subsidy by Federal Employment Agency (=1)	-0.087 (0.312)	0.075** (0.031)	-0.431 (0.282)	-0.295* (0.166)	-0.056* (0.034)	-0.085** (0.037)
<b>IV_06</b>	<b>-5.798***</b> (0.961)					
<b>IV_08</b>	<b>-0.075</b> (0.900)					
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		152.383	11.84	12.17	20.8	20.98
N	1256	1256	1039	851	1291	1272
R-sq.	0.189	0.077	0.073	0.125	0.124	0.151
Mean of dependent variable (abs. value)	4.828	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the **IV** regression of our outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' actual benefit duration (**ABD**) before starting up that is instrumented by *IV06* and *IV08* (Section 1.3.2). Column 1 shows the first-stage regression of the **IV** model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.



**Table 1.16:** IV Results for Reforms 2006 & 2008: ABD on Motivation of Founder and Firm Outcomes for Non-Manufacturing Sector

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>UE Duration (in months)</b>		<b>0.014</b> (0.011)	<b>0.005</b> (0.091)	<b>-0.166**</b> (0.075)	<b>-0.011</b> (0.009)	<b>-0.025**</b> (0.011)
Tertiary degree (=1)	-0.300 (0.356)	-0.083** (0.035)	-0.372 (0.290)	0.153 (0.161)	0.060* (0.035)	0.059 (0.041)
Founder was SE before (=1)	0.252 (0.376)	-0.005 (0.042)	0.096 (0.346)	-0.271 (0.225)	0.015 (0.044)	0.034 (0.050)
Managerial Experience as Employee (=1)	0.160 (0.435)	-0.107** (0.042)	0.005 (0.381)	0.475*** (0.182)	0.129** (0.053)	0.178*** (0.060)
Industry Experience (in years)	0.003 (0.017)	0.001 (0.002)	0.020 (0.016)	0.002 (0.012)	0.002 (0.002)	0.002 (0.002)
Female founder (=1)	0.355 (0.406)	-0.002 (0.042)	-0.830** (0.418)	-0.488** (0.226)	0.071 (0.051)	0.095* (0.055)
Founder of non-German origin (=1)	0.189 (0.552)	-0.033 (0.063)	-1.934*** (0.710)	-0.793* (0.464)	-0.079* (0.046)	-0.079* (0.047)
SE Subsidy by Federal Employment Agency (=1)	-0.086 (0.368)	0.075** (0.035)	-0.637** (0.306)	-0.283 (0.200)	-0.054 (0.035)	-0.069* (0.039)
<b>IV_06</b>	<b>-5.962***</b> (1.111)					
<b>IV_08</b>	<b>-0.103</b> (1.085)					
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic		110.896	10.01	10.13	17.61	17.7
N	999	999	815	661	1022	1009
R-sq.	0.209	0.090	0.078	0.128	0.168	0.169
Mean of dependent variable (abs. value for log-terms)	4.926	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the **IV** regression of our outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' actual benefit duration (**ABD**) before starting up in the non-manufacturing sector (75% of the sample) that is instrumented by *IV06* and *IV08* (Section 1.3.2). Column 1 shows the first-stage regression of the **IV** model in column 2. We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the startup) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix 1.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table 1.17:** Potential Mechanisms: The Role of Selection on Observable Characteristics is limited - Composition and Treatment Effect play a role

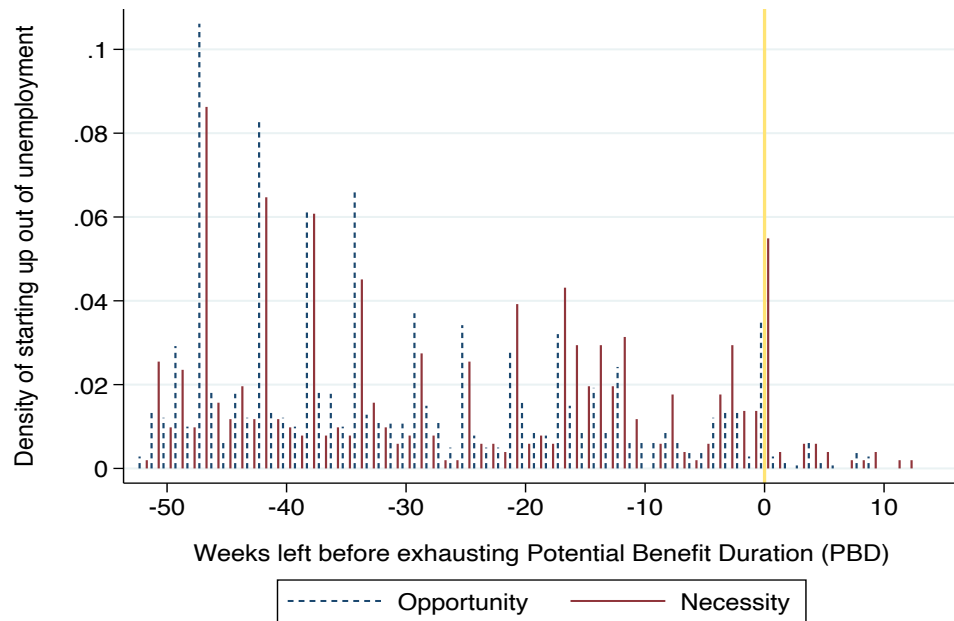
	Treated unemployed from main sample			All unemployed from main sample			"Treated" non-unemployed from comparison group			All non-unemployed from comparison group		
	Mean (before) N=106	Mean (after) N=397	After -before	Mean (before) N=259	Mean (after) N=1032	After -before	Mean (before) N=94	Mean (after) N=509	After -before	Mean (before) N=256	Mean (after) N=1354	After -before
<b>UE Duration (in months)</b>	<b>11.171</b>	<b>4.873</b>	<b>-6.299***</b>	<b>7.943</b>	<b>4.193</b>	<b>-3.75***</b>						
Tertiary degree (=1)	0.33	0.28	-0.05	0.20	0.23	0.03	0.32	0.30	-0.03	0.35	0.25	-0.10*
Founder was SE before (=1)	0.17	0.19	0.02	0.15	0.18	0.03	0.26	0.32	0.05	0.25	0.27	0.01
Managerial Experience as Employee (=1)	0.17	0.15	-0.02	0.13	0.13	0.00	0.16	0.19	0.03	0.14	0.16	0.02
Industry Experience (in years)	20.02	18.81	-1.22	16.10	16.31	0.21	22.71	17.27	-5.44***	17.15	15.38	-1.77
Female founder (=1)	0.17	0.25	0.08	0.13	0.20	0.07	0.18	0.23	0.05	0.14	0.20	0.06
Founder of non-German origin (=1)	0.08	0.04	-0.03	0.04	0.07	0.03	0.08	0.05	-0.02	0.06	0.07	0.01
SE Subsidy by Federal Employment Agency (=1)	0.65	0.80	0.15*	0.65	0.80	0.15***	0.27	0.34	0.07	0.30	0.36	0.06
Technology-intensive services	0.05	0.06	0.01	0.05	0.05	0.00	0.05	0.06	0.01	0.06	0.06	0.00
High-technology manufacturing	0.01	0.01	0.00	0.00	0.01	0.01***	0.01	0.02	0.01	0.01	0.01	0.00
Skill-intensive services	0.06	0.08	0.02	0.03	0.05	0.02	0.08	0.08	0.00	0.09	0.07	-0.02
Software supply and consultancy	0.00	0.01	0.00	0.00	0.01	0.00*	0.01	0.01	0.00	0.01	0.01	0.00
Non-high-tech manufacturing	0.03	0.04	0.01	0.04	0.05	0.01	0.03	0.05	0.02	0.03	0.05	0.02***
Other business-oriented services	0.07	0.11	0.04	0.10	0.14	0.03	0.04	0.12	0.08*	0.12	0.12	-0.01
Cons.-or. services in creative sect.	0.08	0.02	-0.06	0.10	0.05	-0.05	0.24	0.11	-0.13	0.15	0.09	-0.06
Consumer-oriented services	0.30	0.31	0.01	0.26	0.26	0.00	0.15	0.24	0.09	0.14	0.23	0.10*
Construction	0.11	0.20	0.09*	0.19	0.20	0.01	0.12	0.10	-0.02	0.16	0.14	-0.02
Retail & wholesale	0.29	0.17	-0.12	0.21	0.18	-0.03	0.28	0.21	-0.07	0.24	0.22	-0.02
Average daily wage in 5 years before founding	107.87	124.75	16.88	96.87	106.56	9.69	129.17	125.10	-4.06	127.03	119.66	-7.37

*Notes:* Significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table show t-test comparison of different subgroups for the observable characteristics. In the first panel, we compare those unemployed individuals that are affected by 2006 reform. In the second panel, we compare all unemployed individuals before and after the 2006 reform. In the third panel, we compare the potentially treated non-unemployed and in the fourth panel, we do so for all non-unemployed individuals.

## I.1 Appendix: Tables & Figures

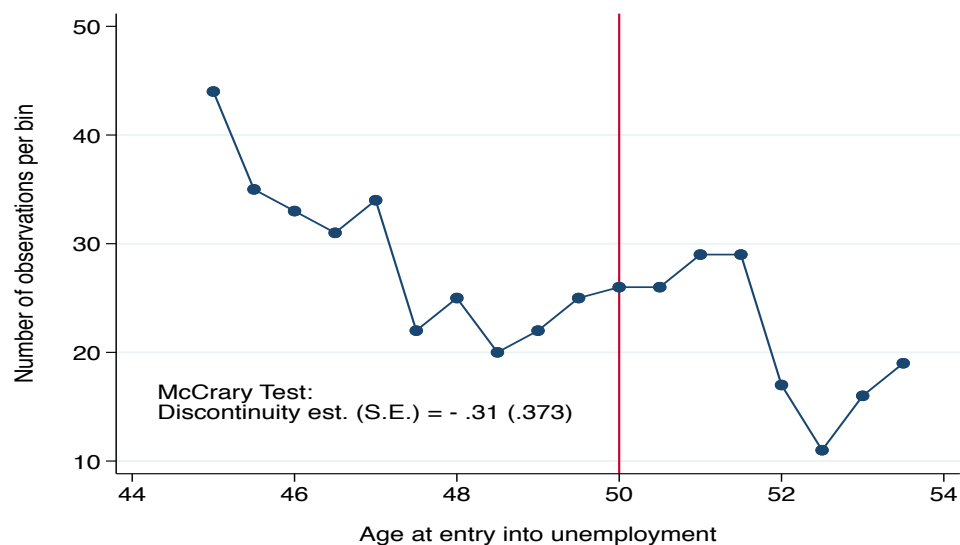
### I.1.1 Figures

**Figure I-1:** Spikes at exhausting Pot. Benefit Duration: Necessity/Pushed Founders

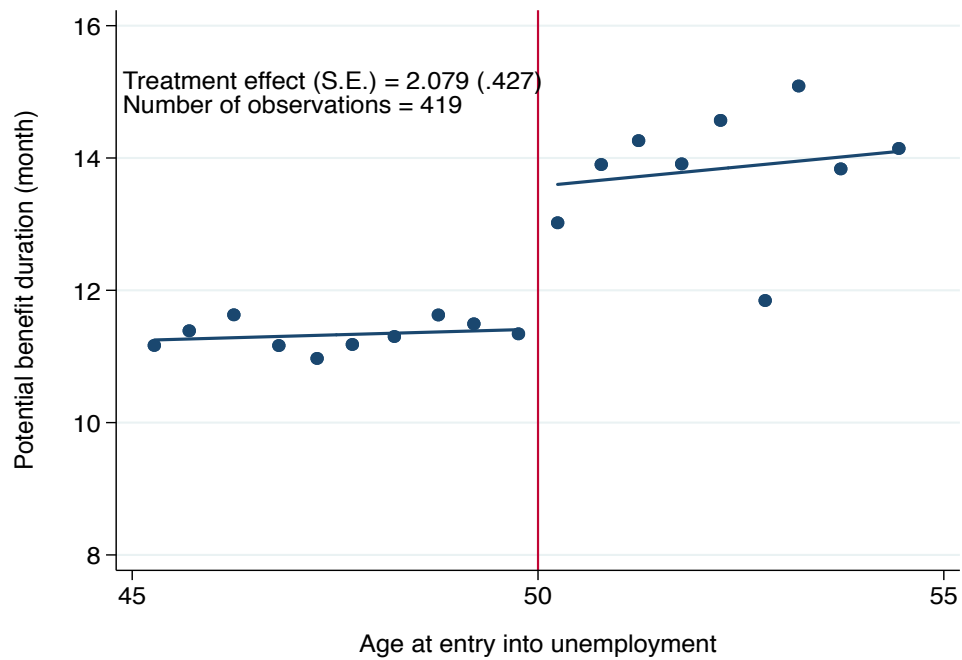
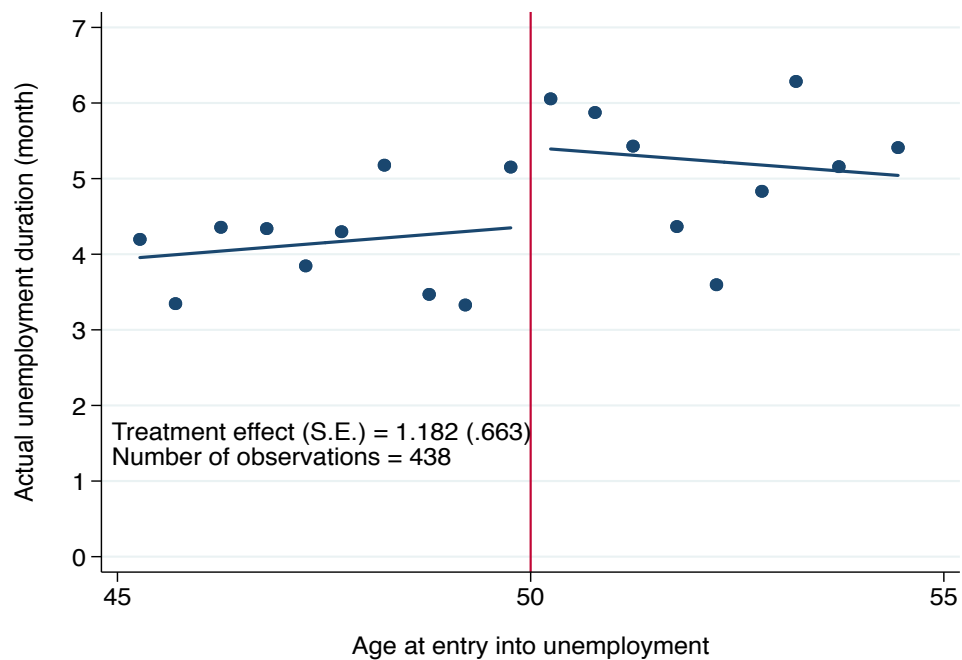


*Notes:* This Figure shows the difference between actual and potential unemployment duration, i.e. when the unemployed individual starts a firm given his/her remaining PBD. The Figure shows that when UI benefits run out (remaining PBD is close to 0) the spike in the exit rate from unemployment to self-employment is significantly higher for those indicating to start a firm due to *necessity* motives (red lines) compared to those indicating an *opportunity* motive (blue dashed spikes). Thus, it is plausible to use the term *pushed* for *necessity*-driven founders (cf. Section 1.2). For a review of the literature on UI spikes, see also Card et al. (2007).

**Figure I-2:** RDD-Strategy McCrary Test for Age 50 Cutoff in 2008-2011



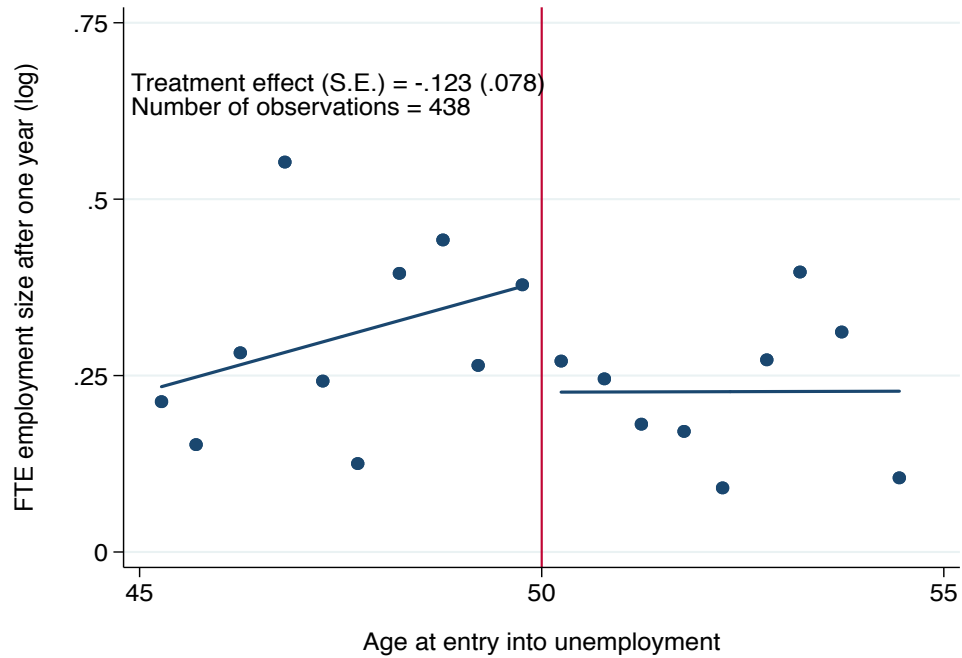
*Notes:* This Figure shows the necessary checks concerning the identifying assumption of the Regression Discontinuity Design (RDD) strategy. This involves conducting a McCrary test that confirms that eligible persons are not strategically becoming unemployed to exploit the age discontinuity in order to optimize their potential benefit duration (PBD). See Section 1.4.2.

**Figure I-3: RDD-Results for Age 50 Cutoff in 2008-2011: Exog. Increase of 3 months in PBD (1)****(a) RDD-Results: Increase in Potential Benefit Duration (PBD)****(b) RDD-Effect of 3 months Increase in PBD on Actual Unemployment Duration**

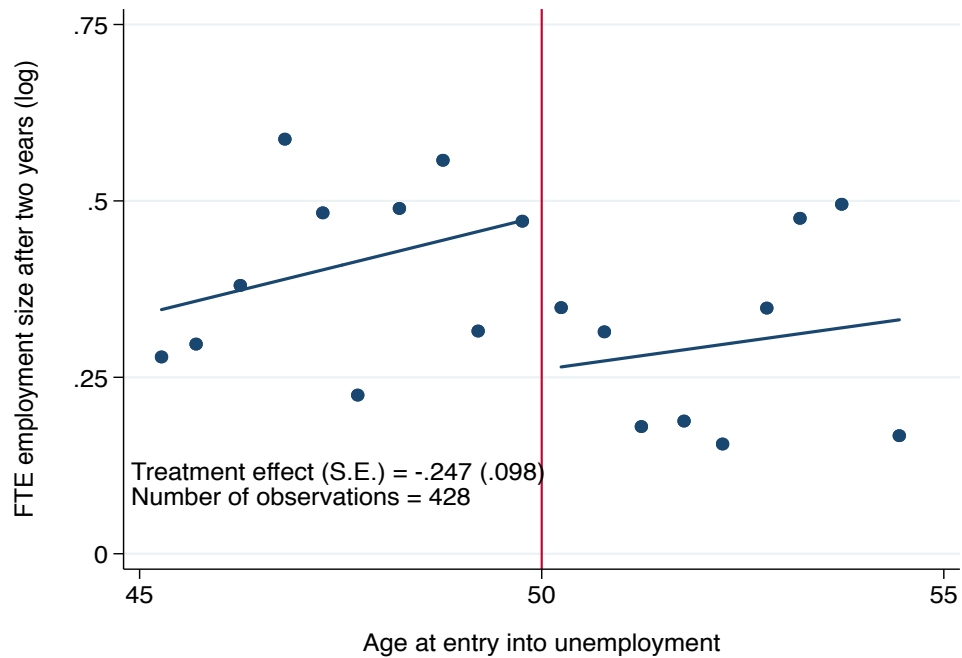
*Notes:* This Figure shows in (a) that potential benefit duration increases by approximately 3 months around the age 50 cutoff, thus it confirms that our data construction process has been successful. Moreover Figure (b) shows regression discontinuity design (RDD) results for the actual unemployment duration of non-team founders (age 45-54) who have become unemployed. Consistent with our IV estimates longer PBD increases actual unemployment duration (ABD). For details on the RDD strategy, see Section 1.4.2.

**Figure I-4:** RDD-Results for Age 50 Cutoff in 2008-2011: Exog. Increase of 3 months in PBD (2)

(a) RDD-Results: Full-Time Equivalent (FTE) Employment after Year 1



(b) RDD-Results: Full-Time Equivalent (FTE) Employment after Year 2



*Notes:* This Figure shows regression discontinuity design (RDD) results for employment growth outcomes of non-team founders (age 45-54) who have been previously unemployed. Consistent with our IV estimates longer actual unemployment duration induced by longer PBD leads to less growth in terms of FTE employment. For details on the RDD strategy, see Section 1.4.2. Detailed results are shown in Table I.1.

## I.1.2 Tables

**Table I.1:** Regression Discontinuity Design (RDD) Results: Exogenous Increase of 3 months in PBD at age 50 cutoff

	(1) PBD (in months)	(2) UE Duration (in months)	(3) FTE Employment Year 1 (log)	(4) FTE Employment Year 2 (log)
<b>RDD Treatment-Effect</b>	<b>2.079***</b> (0.427)	<b>1.182*</b> (0.663)	<b>-0.123</b> (0.078)	<b>-0.247**</b> (0.098)
Tertiary degree (=1)	0.536 (0.333)	0.103 (0.404)	0.048 (0.065)	0.051 (0.060)
Founder was SE before (=1)	-0.614** -0.022	0.720 (0.556)	0.000 (0.058)	0.025 (0.076)
Managerial Experience as Employee (=1)	-0.022 (0.269)	0.188 (0.389)	0.196** (0.071)	0.194** (0.075)
Industry Experience (in years)	0.003 (0.010)	-0.013 (0.021)	0.002 (0.003)	0.001 (0.003)
Female founder (=1)	-0.265 (0.237)	0.233 (0.508)	0.179* (0.096)	0.097 (0.084)
Founder of non-German origin (=1)	0.129 (0.749)	0.248 (0.804)	0.050 (0.076)	-0.037 (0.093)
SE Subsidy by Federal Employment Agency (=1)	0.871*** (0.241)	-0.568 (0.549)	-0.140*** (0.042)	-0.086 (0.058)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes
N	419	438	438	428
R2	0.299	0.083	0.156	0.165
Mean of dependent variable	12.254	4.49	0.279	0.367

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the regression discontinuity design (RDD) results for an exogenous increase of 3 months in PBD at the age 50 cutoff in 2008-2011. This table shows the RDD regression of our outcome variables (PBD, ABD, employment growth after year 1, 2) on an increase of PBD of around 3 months before starting up (Section 1.4.2). We control for the founders' education; previous work experience; and individual characteristics. We include year and industry (of the start-up) fixed effects; and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2008 and 2011, were 45 to 54 years old when becoming unemployed, and for whom information on all control variables is available.

**Table I.2:** DiD Results for 2006 Reform: Reduction of at least 3 months in PBD

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>Treatment-Effect</b> =Treated*After	<b>-3.582***</b> (0.884)	<b>-0.094</b> (0.070)	<b>-0.253</b> (0.494)	<b>0.413</b> (0.345)	<b>-0.041</b> (0.062)	<b>-0.005</b> (0.072)
Treated	4.254*** (0.853)	0.232*** (0.063)	0.142 (0.419)	-0.491 (0.320)	0.043 (0.054)	-0.013 (0.064)
After	-5.091*** (0.605)	-0.087 (0.071)	-0.093 (0.517)	0.417 (0.378)	0.030 (0.062)	0.142* (0.074)
Tertiary degree (=1)	-0.217 (0.294)	-0.068** (0.032)	-0.679** (0.289)	0.007 (0.173)	0.072** (0.034)	0.074* (0.040)
Founder was SE before (=1)	0.061 (0.335)	-0.023 (0.037)	-0.016 (0.343)	-0.082 (0.213)	-0.012 (0.040)	-0.019 (0.045)
Managerial Experience as Employee (=1)	0.179 (0.368)	-0.072* (0.039)	0.150 (0.358)	0.592*** (0.169)	0.131*** (0.050)	0.169*** (0.058)
Industry Experience (in years)	-0.001 (0.015)	0.001 (0.002)	0.034** (0.015)	0.006 (0.010)	0.001 (0.002)	0.001 (0.002)
Female founder (=1)	0.112 (0.337)	0.003 (0.039)	-1.090*** (0.409)	-0.358 (0.218)	0.064 (0.047)	0.102* (0.053)
Founder of non-German origin (=1)	0.253 (0.479)	0.042 (0.060)	-1.891*** (0.676)	-0.572 (0.437)	-0.023 (0.056)	-0.057 (0.057)
SE Subsidy by Federal Employment Agency (=1)	-0.157 (0.297)	0.072** (0.032)	-0.427 (0.282)	-0.285* (0.169)	-0.055 (0.034)	-0.083** (0.038)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1291	1256	1039	851	1291	1272
R-sq.	0.231	0.068	0.098	0.094	0.132	0.133
Mean of dependent variable (abs. value for log-terms)	145.637	0.35	10.074 173,661	11.271 231,293	0.271 0.605	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the DiD regression of our outcome variables (ABD, motivation for starting up; sales/employment growth after year 1, 2) on a decrease of PBD of around 6 months, before starting up (Section 1.4.2). We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the start-up) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.

**Table I.3:** DiD Results for 2006 Reform: Reduction of at least 3 months in PBD focusing on the Non-Manufacturing Sector

	(1) UE Duration (in months)	(2) Necessity Motive (=1)	(3) Sales Year 1 (log)	(4) Sales Year 2 (log)	(5) FTE Employment Year 1 (log)	(6) FTE Employment Year 2 (log)
<b>Treatment-Effect</b> =Treated*After	<b>-3.997***</b> (1.006)	<b>-0.070</b> (0.078)	<b>0.090</b> (0.511)	<b>0.698*</b> (0.415)	<b>0.041</b> (0.058)	<b>0.087</b> (0.071)
Treated	4.885*** (0.971)	0.201*** (0.071)	-0.231 (0.427)	-0.776** (0.391)	-0.033 (0.048)	-0.096 (0.061)
After	-5.004*** (0.686)	-0.032 (0.082)	-0.302 (0.600)	0.442 (0.450)	0.063 (0.062)	0.159** (0.074)
Tertiary degree (=1)	-0.222 (0.337)	-0.084** (0.036)	-0.381 (0.295)	0.178 (0.173)	0.059 (0.036)	0.060 (0.042)
Founder was SE before (=1)	0.203 (0.356)	0.002 (0.042)	0.073 (0.354)	-0.220 (0.233)	0.009 (0.045)	0.029 (0.052)
Managerial Experience as Employee (=1)	0.226 (0.419)	-0.103** (0.043)	0.002 (0.384)	0.412** (0.190)	0.125** (0.054)	0.172*** (0.062)
Industry Experience (in years)	0.006 (0.017)	0.001 (0.002)	0.019 (0.016)	0.003 (0.012)	0.001 (0.002)	0.002 (0.002)
Female founder (=1)	0.290 (0.384)	0.001 (0.043)	-0.829** (0.421)	-0.525** (0.240)	0.069 (0.052)	0.090 (0.057)
Founder of non-German origin (=1)	0.255 (0.517)	-0.032 (0.065)	-1.920*** (0.721)	-0.878* (0.518)	-0.079* (0.047)	-0.086* (0.050)
SE Subsidy by Federal Employment Agency (=1)	-0.142 (0.348)	0.074** (0.037)	-0.634** (0.311)	-0.266 (0.201)	-0.051 (0.036)	-0.064 (0.041)
Industry/Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1022	999	815	661	1022	1009
R-sq.	0.252	0.071	0.080	0.112	0.147	0.141
Mean of dependent variable (abs. value for log-terms)	149.003	0.352	10.21 179,344	11.251 237,112	0.25 0.549	0.329 0.76

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows the DiD regression of our outcome variables (ABD, motivation for starting up; sales/employment growth after year 1, 2) on a decrease of PBD of around 6 months (Section 1.4.2), before starting up in the non-manufacturing sector (75% of the sample). We control for the founders' education, previous work experience, and individual characteristics. We include year and industry (of the start-up) fixed effects, and dummies to control for subsidies from the Federal Employment Agency and for funding by the KfW (Appendix I.3.2). Our regression sample consists of non-team founders who became unemployed between 2003 and 2011, were 35 to 65 years old when becoming unemployed, and for whom information on all control variables is available.



**Table I.4:** OLS - ABD Controlled vs Uncontrolled

	(1) Necessity Motive (=1)		(2) Sales Year 1 (log)		(3) Sales Year 2 (log)		(4) FTE Employment Year 1 (log)		(5) FTE Employment Year 2 (log)	
<b>UE Duration (in months)</b>	<b>0.017***</b> (0.003)	<b>0.017***</b> (0.003)	<b>-0.139***</b> (0.027)	<b>-0.141***</b> (0.028)	<b>-0.096***</b> (0.018)	<b>-0.104***</b> (0.021)	<b>-0.016***</b> (0.003)	<b>-0.018***</b> (0.003)	<b>-0.023***</b> (0.003)	<b>-0.024***</b> (0.003)
<b>CONTROLS</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>
N	1256	1256	1039	1039	851	851	1291	1291	1272	1272
R-sq.	0.063	0.026	0.122	0.028	0.126	0.049	0.150	0.023	0.158	0.034
Mean of dependent variable (abs. value for log-terms)	0.35	0.35	10.074 173,661	10.074 173,661	11.271 231,293	11.271 231,293	0.271 0.605	0.271 0.605	0.361 0.847	0.361 0.847

**Table I.5:** OLS - PBD Controlled vs Uncontrolled

	(1) UE Duration (in months)		(2) Necessity Motive (=1)		(3) Sales Year 1 (log)		(4) Sales Year 2 (log)		(5) FTE Employment Year 1 (log)		(6) FTE Employment Year 2 (log)	
<b>PBD (in months)</b>	<b>0.471***</b> (0.048)	<b>0.498***</b> (0.048)	<b>0.023***</b> (0.003)	<b>0.023***</b> (0.003)	<b>-0.036</b> (0.024)	<b>-0.045*</b> (0.024)	<b>-0.049**</b> (0.022)	<b>-0.053**</b> (0.022)	<b>-0.004</b> (0.004)	<b>-0.008**</b> (0.004)	<b>-0.009**</b> (0.004)	<b>-0.013***</b> (0.004)
<b>CONTROLS</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>
N	1291	1291	1256	1256	1039	1039	851	851	1291	1291	1272	1272
R-sq.	0.256	0.215	0.077	0.044	0.099	0.003	0.096	0.012	0.133	0.004	0.134	0.008
Mean of dependent variable (abs. value for log-terms)	4.785	4.785	0.35	0.35	10.074 173,661	10.074 173,661	11.271 231,293	11.271 231,293	0.271 0.605	0.271 0.605	0.361 0.847	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. These tables show the OLS regression of our main outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' benefit duration (both **ABD** and **PBD**) before starting up. We analyze to which extent controls for founders' education, previous work experience and individual characteristics affect results compared to the standard case without controls. The fact that there are no remarkable differences shows that *composition effects* are limited.

**Table I.6:** IV - ABD Controlled vs Uncontrolled

	(1) UE Duration (in months)		(2) Necessity Motive (=1)		(3) Sales Year 1 (log)		(4) Sales Year 2 (log)		(5) FTE Employment Year 1 (log)		(6) FTE Employment Year 2 (log)	
<b>UE Duration</b> (in months)	<b>-5.815***</b> (0.921)	<b>-5.695***</b> (0.846)	<b>0.022**</b> (0.010)	<b>0.030***</b> (0.010)	<b>0.060</b> (0.095)	<b>0.145</b> (0.092)	<b>-0.118*</b> (0.063)	<b>-0.115*</b> (0.066)	<b>0.005</b> (0.011)	<b>-0.008</b> (0.010)	<b>-0.010</b> (0.012)	<b>-0.017</b> (0.011)
<b>CONTROLS</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>
First-stage F-statistic			39.831	45.318	23.487	26.687	24.016	23.626	40.992	45.866	41.07	45.886
N	1256	1256	1256	1256	1039	1039	851	851	1291	1291	1272	1272
R-sq.	0.189	0.135	0.075	0.021	0.072	.	0.124	0.048	0.120	0.016	0.149	0.032
Mean of dependent variable (abs. value for log-terms)	4.828	4.828	0.35	0.35	10.074 173,661	10.074 173,661	11.271 231,293	11.271 231,293	0.271 0.605	0.271 0.605	0.361 0.847	0.361 0.847

**Table I.7:** IV - PBD Controlled vs Uncontrolled

	(1) PBD (in months)		(2) UE Duration (in months)		(3) Necessity Motive (=1)		(4) Sales Year 1 (log)		(5) Sales Year 2 (log)		(6) FTE Employment Year 1 (log)		(7) FTE Employment Year 2 (log)	
<b>PBD</b> (in months)	<b>-8.743***</b> (0.505)	<b>-8.322***</b> (0.459)	<b>0.661***</b> (0.094)	<b>0.677***</b> (0.094)	<b>0.015**</b> (0.007)	<b>0.020***</b> (0.006)	<b>0.034</b> (0.052)	<b>0.084*</b> (0.048)	<b>-0.072*</b> (0.039)	<b>-0.068*</b> (0.040)	<b>0.003</b> (0.007)	<b>-0.005</b> (0.007)	<b>-0.006</b> (0.008)	<b>-0.012</b> (0.007)
<b>CONTROLS</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>	<b>Yes</b>	<b>No</b>
First-stage F-statistic			299.421	328.397	286.66	319.312	223.603	242.271	220.761	219.543	299.421	328.397	296.11	325.42
N	1291	1291	1291	1291	1256	1256	1039	1039	851	851	1291	1291	1272	1272
R-sq.	0.470	0.434	0.234	0.194	0.083	0.049	0.094	.	0.094	0.011	0.130	0.004	0.134	0.009
Mean of dependent variable (abs. value for log-terms)	12.324	12.324	4.785	4.785	0.35	0.35	10.074 173,661	10.074 173,661	11.271 231,293	11.271 231,293	0.271 0.605	0.271 0.605	0.361 0.847	0.361 0.847

*Notes:* Standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. These tables show the **IV** regression of our outcome variables (motivation for starting up; sales/employment growth after year 1, 2) on the founders' benefit duration (both **ABD** and **PBD**) before starting up that are instrumented by *IV06* (Section 1.3.2). We analyze to which extent controls for founders' education, previous work experience and individual characteristics affect results compared to the standard case without controls. The fact that there are no remarkable differences shows that *composition effects* are limited.

## I.2 Appendix: Technical Details of Data Construction

The **IAB/ZEW** Start-Up Panel is a random sample drawn from the “universe” of the Mannheim Enterprise Panel (“MUP”). The Mannheim Enterprise Panel is collected by Creditreform (Germany’s largest credit rating agency) and processed by **ZEW**. It covers basic information (addresses, phone numbers, industry, incorporation status, survival) about all “economically active” firms in Germany. This is guaranteed by automated synchronization with official commercial registers, automated synchronization with Chambers of Industry and Commerce (IHKs), active search for new firms by local Creditreform offices as well as by the fact that Creditreform usually receives a request for conducting a credit rating when new firms enter the market or require initially investments for starting their business.

Only independent new firms are sampled for the Start-Up Panel survey, which means that all start-ups are included but with a few exceptions: no subsidiaries, no new establishments of established firms, and no firm is included due to a business succession (also in case of insolvency). Instead, joint ventures and franchise are allowed and included. Moreover, a maximum of 75% of the firm may be held by other firms. In conclusion, the interviewee entering into the Start-Up Panel is financially engaged in the firm and usually the single founder or one member of a team of founder(s). The Start-Up Panel is a stratified random sample. The stratification is based on a variable indicating KfW-funding (until 2011), the founding year and the industry sector. The detailed first interview is supposed to take place within one year after the firm has been started. Only a small proportion of firms is first sampled up to three years after foundation to balance small stratification cells (they can be excluded). Shorter follow-up surveys are then conducted in the subsequent years. Each start-up stays in the sample for up to 7 interviews or until they drop out by missing two subsequent interviews. All information is retrieved by computer-assisted telephone interviews. By 2018, survey waves on cohorts 2005-2015 include 68,500 observations from 52,000 interviews on 21,200 firms.

The employment statistics contain information on all reportable employees subject to social security contributions in Germany. This includes apprentices, interns, and people in marginal part-time employment. All notifications on an individual’s spells of employment and unemployment can be linked with the aid of a unique person-specific identifier, thereby revealing an employment history for each employee. A further identifier makes it possible to match the employees to establishments. However, there is no unique identifier to match establishments to firms.

Therefore, we matched establishments to firms in the **IAB/ZEW** Start-Up Panel using a text search algorithm via firm/establishment names and addresses. The text search algorithm is described in detail in Appendix B of [Czarnitzki et al. \(2015\)](#) and has proved to deliver very reliable results in various settings. In the matching procedure we were able to find about 90% of the firms in the **IAB/ZEW** Start-Up Panel that reported having employees in the yearly telephone surveys. We removed firms from the sample which reported that they had reportable employees but which we were unable to find during the matching procedure. In addition, to safeguard against false matches, all matches were double-checked manually and we excluded the matches in the 1<sup>st</sup> and 100<sup>th</sup> percentile of the difference between self-reported and process-generated firm sizes

from the sample. The correlation coefficient between self-reported and process-generated firm sizes in the final firm-year panel dataset is slightly above 0.95.

## I.3 Appendix: Institutional Details

### I.3.1 Labour Market Reforms in Germany

In this first chapter of my dissertation, we exploit parts of the big labor market reforms enacted between 2003 and 2005 that are known as *Hartz-Reforms* (compare e.g. [Hartung et al. \(2018\)](#), [Petrunk & Pfeifer \(2018\)](#) or [Price \(2019\)](#) evaluating these reforms).

- In 2003 the first two parts of this labour market reform were passed: the first measure (*Hartz I*) liberalized the sector of temporary work, thus allowing firms to hire workers from temp agencies for short-term periods. The second measure package (*Hartz II*) reduced the regulations on marginal employment and introduced an additional form of social security tax-favored employment (*mini-jobs*). Moreover, it created new subsidies for unemployed workers starting their own business (*Ich-AG*). But this program ended in 2006 and was replaced by the new startup subsidy scheme (*Gründungszuschuss*, see Appendix [I.3.2](#)).
- In 2004, the third reform package (*Hartz III*) renewed the structure and role of the federal employment agency. Most important, the original placement agencies (*Arbeitsämter*) and social security offices (*Sozialämter*) were merged into single institutions (*Arbeitsagenturen*). Moreover, additional *job centers* were set up in each municipality. Finally, case managers were introduced to have one person in charge of assisting unemployed workers over the entire job search process.
- In 2005, the last reform package (*Hartz IV*) transformed the three-tier system of unemployment benefits, unemployment assistance, and subsistence benefits into a two-tier system of unemployment benefits and subsistence benefits.
  - Concerning the benefit level, the reform involved abolishing unemployment assistance benefits (*Arbeitslosenhilfe*). The unemployment assistance depended on some previous work history and could be received for several years after unemployment insurance ([UI](#)) benefits expired. The net replacement rates were at 53% for a single person and could reach 57% for persons with dependent children. Instead, those who were not eligible for unemployment assistance could still get a minimum subsistence benefit (*Sozialhilfe*) that included rent payments but was not linked to previous wages. The reform of 2005 removed wage-dependent benefits for long-term unemployed, and merged unemployment assistance and subsistence benefit to create a new minimum benefit scheme (*Arbeitslosengeld II*) that is independent of previous wages and only intended to provide recipients with minimum benefits necessary to survive and subject to a tight means testing procedure. Instead, the unemployment insurance ([UI](#)) benefits (*Arbeitslosengeld I*) remained unchanged at a net replacement rate of 60% for single persons and 67% for those with dependent children. Note that we only focus on individuals who enter unemployment and receive [UI](#) benefits, thus, they were not affected by these benefit level changes targeted to those having exhausted [UI](#) benefits.

- Moreover, this package involved changing the duration of unemployment benefits (Table 1.6). The changes came only in effect for individuals claiming for UI benefits after February 1, 2006. In our identification strategy, we exploit this reform along with the 2008 one that partially increased the duration eligibility for older employees again (Lichter 2016). Focusing on 30-60 years old, there is no potential interference with early retirement rules affecting only workers above 60 (ie. 63 since 2004).

### I.3.2 Startup Subsidies

This Section provides an overview of startup subsidies in Germany. The important thing to note is that all the startup subsidies schemes were and are not dependent on the age of claimants and did not change exactly at the same point in time as the reforms on potential unemployment insurance (UI) benefit duration (PBD). Thus, they do not threaten the identification strategy as explained in Section 1.3.

#### 1. Bridging Allowance (BA) - “Überbrückungsgeld” (1986-07/2006)

- Eligibility: it covered individuals who were eligible for unemployment insurance (UI) benefits, and presented an externally approved business plan (issued by the regional chamber of commerce). It was not possible to quit a job and directly apply for this bridging allowance.
- Amount: financial support was based on unemployment insurance (UI) benefits plus social security contributions and it could be provided for up to six months.
- Until 2002, individuals had to stay unemployed for a minimum of one month to apply for BA. From 2002, one could apply for BA from the first unemployment day onward.

#### 2. Existenzgründerzuschuss (Ich-AG) (startup subsidies (SUS)) (01/2003-06/2006)

- Eligibility: it covered individuals who were eligible for unemployment insurance (UI) benefits, but also those with means-tested social assistance or limited labor market experience (hence it was open to more people than BA).
- Amount: it involved a monthly lump-sum payment for up to three years with 600 Euro per month in the first year, 360 Euro per month in the second year, 240 Euro per month in the third year. In contrast to BA, these startup subsidies were approved yearly if self-employment income did not exceed 25,000 Euro per year.
- There was no need of business plans for approval, but parallel receipts of BA and SUS were excluded.

#### 3. New SUS: new startup subsidy program (“Gründungszuschuss”) (08/2006-12/2011)

- Eligibility: it covered individuals that were unemployed for at least one day, eligible to receive unemployment insurance (UI) (Arbeitslosengeld I) and that still had at least 90 days of potential UI benefit duration left when making the transition from unemployment to self-employment. Thus, it was not possible to get this startup subsidy when an unemployed worker just exhausted her UI benefits.

- Amount: it involved **UI** benefits plus 300 Euro (for social security contributions) for nine months. It was possible to get an extension of six months by proving that the business is economically active. The amount of startup subsidies after the first nine months was reduced to just 300 Euro for the remaining six months. In total the startup subsidies could be taken for a maximum of 15 months.
- The first period of SUS could be legally claimed by all persons who fulfilled the legal eligibility requirements. The second period was entirely subject to an assessment.
- In case of returning from self-employment to unemployment, the potential benefit duration (**PBD**) would be reduced by the number of months the person received SUS up to a minimum of zero **PBD** months.

4. New SUS adjusted: startup subsidy program (“Gründungszuschuss”) adjusted (01/2012-today)

- Eligibility: it covers individuals that are unemployed for at least one day, eligible to receive unemployment insurance (**UI**) (Arbeitslosengeld I) and that still have at least 150 days (instead of previously 90 days) of potential **UI** benefit duration left when making the transition from unemployment to self-employment. Thus, it is not possible to get this star-up subsidy when an unemployed worker just exhausts her **UI** benefits.
- Amount: it involves unemployment insurance benefits plus 300 Euro (for social security contributions) for six (instead of previously nine) months. It is possible to get an extension of nine (instead of six) months by proving that the business is economically active. The amount of startup subsidies after the first nine months is reduced to just 300 Euro for the remaining six months. In total, the startup subsidies can be taken for a maximum of 15 months.
- The assessment for receiving startup subsidies has been extended to the first period. The previous legal right to claim this subsidy has been abolished by December 2011 and is now a subsidy that is available upon assessment of the caseworker at the federal employment agency.
- Background: as part of public spending cuts, the intention was to reduce money allocated for such active labor market policies.

It should be noted that we control for any funding provided by the federal employment agency (Bundesagentur für Arbeit) in our regression. Thus, we control for these startup subsidies targeted at founders starting a business out of unemployment.

Moreover, it should be noted that in all regression models, we control for KfW-funding, ie. funding of startups by subsidized credits from the German government-owned development bank, Kreditanstalt für Wiederaufbau (KfW) (Reconstruction Credit Institute) that was formed in 1948 to fund the reconstruction of Germany after World War II. The funding via the KfW is an important channel through which startups in Germany are financed.

## I.4 Appendix: Model Extension - Derivations and Details

### I.4.1 Unemployment Duration on the Value of Searching for Employment

Starting from the modeling framework as discussed in [Section 1.5.1](#) with the value function of an unemployed individual searching for employment ([Equation \(1.8\)](#)), we have:

$$V_{u,t}^e = b_t - \psi_t(s_t) + \beta \left\{ p_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} V_{t+1}^e(w_{t+1}) dF(w_{t+1}) + [p_t F(\phi_t) + (1 - p_t)] V_{t+1}^u \right\} \quad (13)$$

This value function is increasing in the next period's wage  $w_{t+1}$ , such that the reservation wage plays an important role in the optimal search behavior. Every wage that is larger than the reservation wage, i.e.  $w_{t+1} \geq \phi_t$ , will be accepted. Therefore, we can write [Equation \(13\)](#) in terms of the following Bellman equation as:

$$V_{u,t}^e = b_t - \psi_t(s_t) + \beta \left\{ V_{t+1}^u + p_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [V_{t+1}^e(w_{t+1}) - V_{t+1}^u] dF(w_{t+1}) \right\} \quad (14)$$

As mentioned in [Section 1.5.1](#),  $p_t = p(s_t, \theta)$ . The case of leaving unemployment to employment is dependent on the search intensity and an unemployed individual's skill. Holding the level of ability  $\theta$  fix,  $p_t$  is directly dependent on the search intensity  $s_t$ , and can be substituted accordingly. Further, defining the discount factor  $\beta$  as  $\frac{1}{1+\rho}$ , with  $\rho$  being the discount rate (see [Schmieder & von Wachter 2016](#)), one can rewrite [Equation \(14\)](#) to:

$$V_{u,t}^e = b_t - \psi_t(s_t) + \frac{1}{1+\rho} \left\{ V_{t+1}^u + s_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [V_{t+1}^e(w_{t+1}) - V_{t+1}^u] dF(w_{t+1}) \right\} \quad (15)$$

Assuming that the value of unemployment is in equilibrium equal to the discounted reservation wage, we define  $V_{t+1}^u = \frac{1}{\rho} \phi_t$ . Analogously, the value of leaving unemployment in the next period depends on the present value of the reservation wage  $V_{u,t+1}^e = \frac{1}{\rho} \phi_t$ , or in this case  $V_{u,t}^e = \frac{1}{\rho} \phi_{t-1}$ .

Note that the individual is indifferent between leaving unemployment or staying unemployed at the exact level of the reservation wage when  $V_{u,t+1}^e = V_{t+1}^u$ . Knowing the reservation wage  $\phi_t$  and the optimal search intensity  $s_t$  in period  $t$  will enable us to detect the reservation wage in period  $t - 1$ . Therefore, plugging in  $V_{t+1}^u = \frac{1}{\rho} \phi_t$ ,  $V_t^u = \frac{1}{\rho} \phi_{t-1}$  into [Equation \(15\)](#), we get:

$$\frac{1}{\rho} \phi_{t-1} = b_{t-1} - \psi_t(s_t) + \frac{1}{1+\rho} \left\{ \frac{1}{\rho} \phi_t + s_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} \left[ V_{t+1}^e(w_{t+1}) - \frac{1}{\rho} \phi_t \right] dF(w_{t+1}) \right\} \quad (16)$$

Multiplying [Equation \(16\)](#) by  $\rho(1+\rho)$ , we get:

$$(1+\rho)\phi_{t-1} = (1+\rho)\rho(b_{t-1} - \psi_t(s_t)) + \phi_t + s_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [\rho V_{t+1}^e(w_{t+1}) - \phi_t] dF(w_{t+1}) \quad (17)$$

To find the optimal reservation wage, we need to derive the first-order conditions. With the optimal reservation wage implying indifference between the value functions for searching employment and for remaining unemployed (when  $V_{u,t+1}^e = V_{t+1}^u$ ), we can further solve for the optimal search intensity (taking the derivative of Equation (17) for  $s_t$  at the reservation wage  $\phi_{t-1}$ ):

$$(1 + \rho)\rho\psi'_t(s_{t-1}) - [1 - F(\phi_{t-1})] \int_{\phi_{t-1}}^{\infty} [\rho V_t^e(w_t) - \phi_{t-1}] dF(w_t) = 0 \quad (18)$$

As mentioned in the main text, an unemployed individual receives a constant benefit  $b_t$  for a duration  $d$ . When  $t \geq d = \text{PBD}$ , the benefit drops to a lower and constant level. This illustrates the importance of duration  $d$ . Since the reservation wage and the optimal search intensity are two choice variables that directly influence the value for employment, we are interested in their behavior over the unemployment duration spell  $d$ .

Exploiting the fact  $V_{t+1}^u = \frac{1}{\rho}\phi_t$ , the first order condition for the optimal reservation path is given by:

$$\frac{\partial \phi_t}{\partial d} = \frac{\partial V_{t+1}^u}{\partial d} \rho \quad (19)$$

Taking the total derivative of Equation (18) with respect to  $d$ , we get for the optimal search intensity path at period  $t$ :

$$\frac{\partial s_t}{\partial d} = - \frac{\partial \phi_t}{\partial d} \frac{[1 - F(\phi_t)]^2 + f(\phi_t) \int_{\phi_t}^{\infty} [\rho V_t^e(w_t) - \phi_t] dF(w_t)}{(1 + \rho)\rho\psi''_t(s_t)} \quad (20)$$

If there exists at least the slightest chance someone cannot find a job by the time unemployment benefits expire i.e  $t = d = \text{PBD}$ , then a longer benefit duration in general increases the value for unemployment i.e  $\frac{\partial V_{t+1}^u}{\partial d} > 0$ . Equation (19) and Equation (20) show that a longer  $d$  will lead to a higher reservation wage  $\phi_t$  and a lower search intensity  $s_t$ .

This means that given the hazard of leaving unemployment is given as  $h_t = s_t(1 - F(\phi_t))$ , an extension of  $\text{PBD}$  would lower the probability of leaving unemployment in that period, thus increasing actual unemployment duration (compare also Schmieder & von Wachter 2016). Moreover, this implies that the effect of unemployment duration on the value of searching for employment should be negative

$$\frac{\partial V_{ut}^e}{\partial d} \Big|_{\theta} < 0 \quad (21)$$

In other words, this model implies negative duration dependence which leads to the implications as described in the main text in Section 1.5.



### I.4.2 Effect of Unemployment Duration on the Value of Self-Employment

Starting from the modeling framework as discussed in [Section 1.5.1](#) with the value function of an unemployed individual searching for employment ([Equation \(1.10\)](#)), we have:

$$V_{u,t}^{se} = b_t - \psi_t^{se}(s_t, \theta) + \beta \left\{ p_t [1 - F(\phi_t)] \int_{\phi_t}^{\infty} V_{t+1}^{se}(\pi_{t+1}) dF(\pi_{t+1}) + [p_t F(\phi_t) + (1 - p_t)] V_{t+1}^u \right\} \quad (22)$$

Giving the same importance to the reservation wage and using the same definition for  $\beta$  as in the case before, holding  $\theta$  fixed we can rewrite the above equation in terms of the following Bellman equation as:

$$V_{u,t}^{se} = b_t - \psi_t^{se}(s_t) + \beta \left\{ V_{t+1}^u + p_t^{se} [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [V_{t+1}^{se}(\pi_{t+1}) - V_{t+1}^u] dF(\pi_{t+1}) \right\} \quad (23)$$

This value function is again increasing in the next period's profits as income when being self-employed  $\pi_{t+1}$ . Every potential profit as self-employed that is larger than the reservation wage, i.e  $\pi_{t+1} \geq \phi_t$ , will be accepted. Note that the search costs have a different interpretation in this case.  $\psi^{se}$  reflects costs related to developing a startup idea, doing the required research on it or finding capital. Furthermore, defining the discount factor  $\beta$  as  $\frac{1}{1+\rho}$ , with  $\rho$  being the discount rate, one can rewrite [Equation \(23\)](#) to:

$$V_{u,t}^{se} = b_t - \psi_t^{se}(s_t) + \frac{1}{1+\rho} \left\{ V_{t+1}^u + p_t^{se} [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [V_{t+1}^{se}(\pi_{t+1}) - V_{t+1}^u] dF(\pi_{t+1}) \right\} \quad (24)$$

For the case of moving from unemployment to self-employment, we define  $V_{u,t+1}^{se} = \frac{1-\gamma(\bar{\theta})}{\rho} \phi_t$ : i.e it depends also on the probability of startup success (survival), here for the average type at  $\bar{\theta}$ . Note, that the individual is indifferent between leaving unemployment or staying unemployed at the exact level of the reservation wage when  $V_{u,t+1}^{se} = V_{t+1}^u$ . Knowing the reservation wage  $\phi_t$  and the optimal search intensity  $s_t$  in period  $t$  will enable us to detect the reservation wage in period  $t-1$ . Plugging in  $V_{t+1}^u = \frac{1-\gamma(\bar{\theta})}{\rho} \phi_t$ ,  $V_t^u = \frac{1-\gamma(\bar{\theta})}{\rho} \phi_{t-1}$  into [Equation \(24\)](#), we get:

$$\begin{aligned} \frac{1-\gamma(\bar{\theta})}{\rho} \phi_{t-1} &= b_{t-1} - \psi_t^{se}(s_t) \\ &+ \frac{1}{1+\rho} \left\{ \frac{1-\gamma(\bar{\theta})}{\rho} \phi_t + s_t^{se} [1 - F(\phi_t)] \int_{\phi_t}^{\infty} \left[ V_{t+1}^{se}(\pi_{t+1}) - \frac{1-\gamma(\bar{\theta})}{\rho} \phi_t \right] dF(\pi_{t+1}) \right\} \end{aligned} \quad (25)$$

After some rearranging we get:

$$\begin{aligned} [1 - \gamma(\bar{\theta})](1 + \rho) \phi_{t-1} &= (1 + \rho) \rho (b_{t-1} - \psi_t^{se}(s_t)) \\ &+ (1 - \gamma(\bar{\theta})) \phi_t + s_t^{se} [1 - F(\phi_t)] \int_{\phi_t}^{\infty} [\rho V_{t+1}^{se}(\pi_{t+1}) - (1 - \gamma(\bar{\theta})) \phi_t] dF(\pi_{t+1}) \end{aligned} \quad (26)$$

To find the optimal reservation wage, we again need to derive the first-order conditions. With the optimal reservation wage implying indifference between the value functions for becoming self-employed and for remaining unemployed (when  $V_{u,t+1}^{se} = V_{t+1}^u$ ), we can further solve for the optimal search intensity (taking the derivative of Equation (26) for  $s_t$  at the reservation wage  $\phi_{t-1}$ ):

$$(1 + \rho)\rho\psi_{t-1}^{se}(s_{t-1}) - [1 - F(\phi_{t-1})] \int_{\phi_{t-1}}^{\infty} [\rho V_t^{se}(\pi_t) - (1 - \gamma(\bar{\theta}))\phi_{t-1}] dF(\pi_t) = 0 \quad (27)$$

As mentioned in the main text, an unemployed individual receives a constant benefit  $b_t$  for a duration  $d$ . When  $t \geq d = \text{PBD}$ , the benefit drops to a lower and constant level. This illustrates the importance of duration  $d$ . Since the reservation wage and the optimal search intensity are two choice variables that directly influence the value for employment, we are interested in their behavior over the unemployment duration spell  $d$ .

Exploiting the fact  $V_{t+1}^u = \frac{(1-\gamma(\bar{\theta}))}{\rho}\phi_t$ , the first order condition for the optimal reservation path is given by:

$$\frac{\partial \phi_t}{\partial d} = \frac{\partial V_{t+1}^u}{\partial d} \frac{\rho}{(1 - \gamma(\bar{\theta}))} \quad (28)$$

Taking the total derivative of Equation (27) with respect to  $d$ , we get for the optimal search intensity path:

$$\frac{\partial s_t}{\partial d} = - \frac{\frac{\partial \phi_t}{\partial d} [1 - F(\phi_t)]^2 + f(\phi_t) \int_{\phi_t}^{\infty} [\rho V_t^{se}(\pi_t) - (1 - \gamma(\bar{\theta}))\phi_t] dF(\pi_t)}{(1 + \rho)\rho\psi_t'(s_t)} \quad (29)$$

Again when unemployment benefit duration increases and there is chance of not finding a job when benefits expire, we expect that  $\frac{\partial V_{t+1}^u}{\partial d} > 0$ . With  $\rho$  being the discount rate taking values  $< 1$  and  $[1 - \gamma(\bar{\theta})]$  being the probability of startup success that is higher than  $\rho$ , the whole fraction is  $\frac{\rho}{(1-\gamma(\bar{\theta}))} < 1$ . The denominator becomes larger the higher an individual's ability  $\theta$ . Equation (28) and Equation (29) show that searching market opportunities for self employment exhibits a smaller unemployment duration dependence of the reservation wage path ( $\frac{\partial \phi_t}{\partial d}$ ) and also a less negative duration dependence of the search intensity path ( $\frac{\partial s_t}{\partial d}$ ).

This implies that given Equation (22) the effect of unemployment duration on the value of searching for self-employment should be negative but less than in the case of searching for employment (compare Equation (21))

$$\frac{\partial V_{ut}^e}{\partial d} | \theta < \frac{\partial V_{ut}^{se}}{\partial d} | \theta < 0 \quad (30)$$

In other words, this model implies negative duration dependence when searching for self-employment out of unemployment, however, it is less negative than the duration dependence when searching for employment. This leads to the implications as described in the main text in Section 1.5. Thus,  $\frac{\partial V_{ut}^{se}}{\partial d}$  is larger than in the case of searching for employment, i.e. there is a higher UI duration elasticity - which is in line with our empirical results in Section 1.4.

## **Chapter 2**

# **Unemployment Benefits and the Transition into Self-Employment**

## 2.1 Introduction

Reducing unemployment is a common public policy goal which becomes especially important during a period of economic crisis. For this reason, unemployment insurance (UI) policies aim to provide a social safety net while limiting moral hazard to promote re-employment and reduce time in unemployment. In this context, most studies analyze how the generosity of UI systems in terms of potential benefit duration (PBD) and replacement rates (i.e. benefit levels) affects re-employment outcomes (Solon 1985, Katz & Meyer 1990b, Card & Levine 2000, Kolsrud et al. 2018, etc.). However, this focus neglects another typical channel of leaving unemployment: the transitions into self-employment (SE). This post-unemployment outcome which accounts for 10-15% of the labor force in the member countries of the Organization of Economic Co-operation and Development (OECD)<sup>1</sup> is economically relevant since one quarter of all new firms is started out of unemployment each year.<sup>2</sup> Therefore, given the potential of successful startups to sustainably create additional employment or boost innovation, and because self-employment is a typical trajectory for individuals to exit unemployment, it is important to understand the role of UI benefits on the transition from unemployment to self-employment. This is necessary to complete the analysis of how the design of unemployment benefits affects all relevant post-unemployment outcomes (and not only dependent employment), and thus may lead to more efficient unemployment policies.

The second chapter of my doctoral thesis aims to shed light on this issue by analyzing how UI benefit levels affect the probability of unemployed individuals to become self-employed, as well as their actual unemployment duration before transitioning to self-employment in comparison to the case of re-employment.<sup>3</sup> Exploiting reform-driven exogenous variation in UI benefit levels, we are among the first to estimate the causal effect of UI benefits (holding PBD fixed) on total employment, and decompose the overall effect into the causal effects on transitions from unemployment to self-employment and to re-employment. Since most other studies investigate increases in UI generosity, our focus on analyzing a reduction in UI benefit levels is also novel within this field of research.

To answer the research questions mentioned above, we focus on the Spanish UI system and use comprehensive social insurance data linked with income tax data from the Continuous Working Life Sample (*Muestra Continua de Vidas Laborales*) (MCVL). In particular, we prepare the administrative data to include so far inaccessible information on self-employment because this is necessary to analyze our variables of interest.

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<sup>1</sup>Spain is particularly interesting because its self-employment rate is among the highest in the European Union (EU). Spain's self-employment rate has been between 16.4% and 17.9% within the last decade (OECD 2018).

<sup>2</sup>In Spain, around 30-50% of founders between 2005 to 2017 have been unemployed before starting their firms.

<sup>3</sup>Concerning the notation in this chapter: with *self-employment*, we refer to the labor market status to distinguish unemployment, employment and self-employment. Within the labor market status of *self-employment*, the term *founder* refers to the person starting a firm which covers both firms with and without employees. The term *entrepreneur* is used to focus on a founder who continues to run a firm after having started it. The term *startup* refers to the act of starting a firm and is used as a synonym for *new firm*.

First, we document the evolution of all relevant labor market status flows over the business cycle (2005-2017) in Spain, and analyze the relevance of flows from unemployment into self-employment. Second, our causal analysis focuses on the direct link between the reduction of **UI** benefits and the probability to become self-employed (compared to becoming re-employed) in a setting where the **PBD** schedule of the **UI** system remained fixed. In 2012, the Spanish government implemented a labor market reform which led to a sharp change in **UI** benefits: it decreased **UI** benefit levels by 10 percentage points (from a replacement rate of 60% to one of 50%) for all eligible individuals with a **PBD** surpassing six months. This quasi-experimental set-up allows us to exploit exogenous variation in our explanatory variable of interest, the **UI** benefit level, in order to estimate the causal effect of a reduction of **UI** benefits on the probability to become either self-employed or re-employed as dependent employee. In this context, we decompose the total reform effect on the average actual unemployment duration into the effect on individuals who become either self-employed or re-employed, and we calculate distinct **UI** benefit level duration elasticities. We apply both a Difference-in-Differences (**DiD**) approach and a Regression Discontinuity Design (**RDD**) to estimate our causal effects. The **DiD** estimation allows us to study not only the average treatment effect, but also the dynamic reform effects on treated relative to untreated individuals over the unemployment spell. This enables us to analyze the behavioral responses to **UI** benefits with respect to both job search and startup efforts. The **RDD** approach, which relies on the time interval between the **UI** entry date and the sharp reform cutoff date, confirms the internal validity of our identification strategy: it shows that manipulation around the cutoff date is not an issue because the reform could not be anticipated due to its unexpected implementation.

Regarding the causal effects of the reform, we find that in response to the cut in **UI** benefit levels the self-employment probability is estimated to be rather unaffected in the short run. In the medium and long run, this effect tends to become negative. On the contrary, the probability of finding a job is rather positively affected in the short run while flattening out in the medium and long run. The total employment effect is thus rather slightly positive in the short run, but attenuates towards zero after two years. These results clearly show a behavioral response of the affected individuals. In response to the reform, treated unemployed individuals increase their search intensity to find employment before **UI** benefits drop after six months. This explains the increase in the short-run employment probability and its decline after the first six months. Instead, when we consider self-employment as an additional exit channel out of unemployment, this response seems to be much smaller. In relative terms, the self-employment probability declines compared to the job-finding probability.

We estimate that the **UI** benefit duration elasticity is around 0.4 (in the **DiD** setting) and 0.5-0.66 (in the **RDD** setting) for those becoming re-employed, which is slightly higher compared to findings in other studies of the literature. Interestingly, we find the **UI** benefit duration elasticity to be smaller (around 0.26-0.33 in the **DiD** setting and 0.11-0.38 in the **RDD** setting) for those transitioning from unemployment to self-employment.

Thus, **UI** benefit levels affect the actual unemployment duration of unemployed individuals no matter whether they become re-employed or self-employed. However, the effect is stronger for re-employment than self-employment, i.e. reducing **UI** benefit levels reduces actual **UI** duration more for those transitioning into employment than those becoming self-employed. On the macro level, our results suggest that the cut in **UI** benefit levels shifts the transition from unemployment towards employment rather than self-employment. Finally, our descriptive analysis illustrates that in response to the reform new firms are predominantly created in the service sector, whereas the share of startups in the industry and construction sector declines. This indicates that in addition to the effects on the extensive margin, the quality of self-employment may be affected. Less generous **UI** benefits may not only decrease transitions from unemployment to self-employment, but also increase the share of *necessity-driven* entrepreneurship among previously unemployed founders. Therefore, we try to disentangle the causal reform effect on different measures of self-employment quality to obtain evidence for the potential welfare effect. However, as both of our quasi-experimental models produce only insignificant results, we cannot confirm the causal nature of the welfare effects. Thus, more research is needed to assess the potential welfare implications of **UI** benefits on self-employment.

This chapter of my dissertation relates to three strands of the literature, and makes three contributions. First, we contribute to the entrepreneurship literature by providing evidence on the role of **UI** benefits for entrepreneurship (Evans & Leighton 1989a, Levine & Rubinstein 2017, etc.). Using administrative data from social insurance and tax authorities in Spain, we enable analyzing the unemployment exit channel into entrepreneurship over time (and business cycle), and show that inflows into self-employment from unemployment can account for up to 50% of all new businesses (in times of crisis).

Second, this project adds to the literature in public economics on the optimal design of unemployment insurance; especially, by providing evidence for the effect of **UI** benefits on the transitions into re-employment and self-employment. The public economics literature has discussed several aspects concerning the optimal design of unemployment insurance policies, i.e. the level of benefits and their eligible duration (Schmieder et al. 2012, 2016, Kolsrud et al. 2018, etc.). Its focus has been on investigating effects on subsequent employment outcomes, predominantly re-employment wages (Schmieder et al. 2016, Nekoei & Weber 2017). Results suggest that increases in **UI** benefit levels lead to increases in actual unemployment duration. However, the effects of longer actual unemployment on re-employment wages are disputed. For instance, Nekoei & Weber (2017) argue that longer **PBD** can either induce delay in job acceptance and simply subsidize leisure, or improve job opportunities through promoting a longer search that results in job matches of higher quality. While Nekoei & Weber (2017) find that the latter positive effect dominates in Austria, Schmieder et al. (2016) report negative effects of unemployment durations on re-employment wages in Germany. We contribute to the debate by providing first evidence on the causal effect of cutting unemployment benefit

levels on self-employment. Thus, this chapter complements the analysis of UI benefits regarding post-unemployment outcomes (Jäger et al. 2019). We show that UI benefits generate a *fiscal externality* (Lawson 2017) through the transition from unemployment to self-employment. This should be taken into account for the optimal design of UI systems.

Third, we contribute to the literature on (un)intended consequences of economic crisis politics. In fact, the labor market reform that we analyze was one of the policies to deal with the Spanish crisis and was supposed to reduce unemployment under the pressure of fiscal consolidation. Thus, we contribute to the literature on extending UI generosity during times of crisis which has mostly focused on the US (e.g. Farber et al. 2015, Card et al. 2015) because we provide evidence on how the non-standard response of cutting UI benefits in a crisis period affects both re-employment and self-employment. Therefore, we also contribute to the limited literature on reducing UI generosity instead of increasing it. For instance, Rebollo-Sanz & Rodríguez-Planas (2020) or Doris et al. (2018) find that cuts in UI benefits can increase the job-finding rate. Instead, we are the first to investigate the effect of a cut in UI benefits on the self-employment (start-up) rate. Moreover, this chapter relates to the work of Hombert et al. (2020) who exploit a French reform in 2002 which lowered the downside risk to start a business. They find that more self-employment is created when more social security is provided. In contrast, our contribution is to analyze the causal effect of providing less security (less UI benefits) on self-employment (instead of employment), and on the UI benefit duration elasticity.

The case of Spain is especially interesting as its external validity is higher compared with inference from other European countries with good data access on self-employed individuals (e.g. Scandinavian countries whose labor markets are smaller). Our research questions appear to be of high relevance in times of high unemployment rates (as in Spain and other European countries during the 2010s).<sup>4</sup> Moreover, we can learn about the general bias created in studies which focus only on employment and give insights on the full picture of inflows into self-employment.

The chapter proceeds as follows. Section 2.2 provides the theoretical background in relation to the literature and discusses potential determinants of the self-employment probability. Section 2.3 illustrates the data used and presents a descriptive analysis of the Spanish labor market and all labor market flows over time (2005-2017). Section 2.4 describes the institutional setting of the unemployment benefit system in Spain, as well as the investigated labor market reform on which our identification strategy relies. In Section 2.5, we explain our estimation methodology and its underlying assumptions. Section 2.6 presents results, while Section 2.7 discusses their interpretation (with respect to the theory) and potential policy implications (welfare). Section 2.8 concludes.

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<sup>4</sup>Current active labor market policies in Europe increasingly subsidize unemployed individuals to start their own businesses. Especially in Spain, such policies have been used to address the high (youth) unemployment rates after the economic crisis of 2007/2008. For instance, in 2013 the Spanish government launched the *Strategy of Entrepreneurship and Youth Employment 2013-2016*. This program aimed at promoting self-employment among the unemployed youth through reductions in social security contributions (González Menéndez & Cueto 2015).



## 2.2 Theory

### 2.2.1 Literature Review

The labor market literature has already extensively discussed the implications of changes in UI generosity on the probability of leaving unemployment in favor of employment (see [Atkinson & Micklewright 1991](#), for a critical literature review). However, there is a lack of literature which deals with the link between UI generosity and self-employment. This research field is related to two strands of literature: Standard Search Theory and the Entrepreneurial Choice Model.

**Standard Search Theory.** [Mortensen \(1977\)](#) provides a general framework to investigate the relationship between UI generosity and the probability of leaving unemployment. A representative individual maximizes the present value of lifetime utility based on the expected income<sup>5</sup> from her labor market status and leisure. The probability of leaving the UI benefit spell depends, on the one hand, positively on the worker's search intensity<sup>6</sup>, and on the other hand, negatively on her reservation wage. If either the UI benefit level or the entitlement period is increased, the opportunity cost of searching will rise as well, thus creating disincentives to find employment quickly ([Schmieder et al. 2016](#)). Hence, both an increase in the UI benefit level and in the UI potential benefit duration lead to a lower probability of leaving unemployment. Conversely, unemployed individuals ineligible to benefits will experience an entitlement effect. Their expected future utility from UI benefits will increase, which gives them an incentive to work so that they become qualified for future UI benefits. As a consequence, their probability of leaving unemployment increases ([Mortensen 1977](#)).

In the case of Spain, [Bover et al. \(2002\)](#) use exogenous variation in the receipt of UI benefits in order to disentangle the effect of benefits on unemployment duration for male workers. They exploit a labor market reform in 1984 which generated a new worker type who was ineligible for any UI benefits. The authors find that if unemployed individuals receive benefits, their hazard of leaving unemployment will significantly decline, which is in line with Standard Search Theory.

According to [Reize \(2000\)](#) Standard Search Theory offers a framework to investigate the search behavior of unemployed individuals and their decision to leave unemployment, regardless of the destined labor market status. However, most authors use this theory to analyze the transition into re-employment. For instance, [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#) consider a Spanish labor market reform in 2012 which decreased the UI benefit amount after the first six months of the UI benefit period. Supporting the findings of [Bover et al. \(2002\)](#), they find that the benefit reduction shortens the mean expected

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<sup>5</sup>Income equals earnings if the individual is (self-)employed and it equals the unemployment benefit amount if the individual receives UI benefits.

<sup>6</sup>A higher search intensity will increase the likelihood of receiving a job offer and, thus, increase the probability of leaving the UI benefit spell ([Mortensen 1977](#)).



unemployment duration by 14%. Additionally, the authors show that the benefit reduction increased the employment probability of unemployed individuals by 41% compared to workers unaffected by the reform, using both a DiD approach and a RDD. The authors also investigate potential responses in the job search behavior to the reform. They find most of the reform effect is captured by individuals adapting their search behavior before the UI benefit level is actually reduced after six month (Rebollo-Sanz & Rodríguez-Planas 2020). Our analysis is based on exploiting the same reform, but with regards to analyzing the transition into self-employment and taking into account longer-term responses (up till 2017, and not only until 2014).

**Entrepreneurial Choice Model.** This theoretical framework originated from the findings of Lucas (1978), Kihlstrom & Laffont (1979), and Evans & Jovanovic (1989). Individuals compare their expected returns from employment and self-employment, and choose the status with the larger expected net income. The basic models focus on certain aspects of the entrepreneurial choice problem but they do not take into account the broad range of possible determinants for starting up, such as being unemployed.

Firstly, Lucas (1978) focuses on entrepreneurial skills.<sup>7</sup> He finds that if an individual's entrepreneurial skill exceeds a certain threshold, he or she will become an entrepreneur, whereas individuals with an entrepreneurial ability below this threshold will switch into employment. Secondly, Kihlstrom & Laffont (1979) consider the impact of an individual's risk attitude on the entrepreneurial choice.<sup>8</sup> They show that in equilibrium, less risk averse individuals choose to become self-employed, whereas risk averse individuals choose employment. Lastly, Evans & Jovanovic (1989) were the first to account for two potential determinants in a model of entrepreneurial choice: capital constraints and entrepreneurial skills.<sup>9</sup> According to the authors, individuals with a lack in entrepreneurial skills will not select into self-employment. Individuals whose skills are large enough will become self-employed and invest into their businesses according to their liquidity constraints.<sup>10</sup> Evans & Jovanovic (1989) conclude that without liquidity constraints, more people would enter self-employment and invest more efficiently.

Regarding Spain, Alba-Ramirez (1994) expanded the Entrepreneurial Choice Model and included unemployment. According to the author, the self-employment probability decreases with the increase of UI benefit spell duration. He argues that job search is costly because human capital deteriorates during the state of unemployment. Therefore, the longer the spell continues, the more an individual's reservation wage decreases, which would also lower the expected utility from employment. Additionally, the author states

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<sup>7</sup>Liquidity constraints are ignored and all individuals are assumed to be risk-neutral and to have the same employment skills (Lucas 1978).

<sup>8</sup>Entrepreneurial ability and wealth are assumed to be equal across individuals (Kihlstrom & Laffont 1979).

<sup>9</sup>Risk preferences are ignored (Evans & Jovanovic 1989).

<sup>10</sup>One group has enough entrepreneurial skills but is liquidity constrained, as they have not enough initial assets and, thus, their access to capital is constrained. These entrepreneurs will invest an inefficiently low amount of money. Another group has enough entrepreneurial skills and is unconstrained because they have access to enough initial assets (Evans & Jovanovic 1989).

that job search takes place in a learning environment. The longer the **UI** spell duration, the more the individual will learn about his or her market opportunities. Caused by this learning environment, entrepreneurial ability increases and self-employment prospects improve. Consequently, the self-employment probability rises. If the expected income from self-employment exceeds the expected income from employment, the individual stops searching for employment and instead focuses on starting a business. [Alba-Ramirez \(1994\)](#) estimates the determinants of the self-employment probability in a sample of previously employed workers and **UI** recipients. Given the individual was a former **UI** recipient, the author finds that the probability for self-employment significantly increases with longer **UI** spell duration. Unfortunately, his estimates may suffer from selection bias as individuals who remain **UI** benefit recipients are not taken into account.

### 2.2.2 Hypotheses

The Spanish labor market reform in 2012 decreased the level of **UI** benefits received after the first six months of the benefit spell. Based on the previous literature review, different hypotheses can be derived about the effect of a reduction in **UI** benefits on the self-employment probability.

**Standard Search Theory Hypothesis.** According to this hypothesis, the drop in **UI** benefit levels may lead to an increase in the self-employment probability, already at the beginning of the **UI** benefit spell. In particular, if the **UI** benefit level is decreased after six months, this will lower the reservation wage and opportunity costs of searching will decline. Since individuals could anticipate the impending drop in the **UI** benefit level, the search intensity for job or market opportunities will increase already at the beginning of the **UI** benefit spell. Consequently, the probability of leaving unemployment would increase and, thus, unemployment duration would become shorter.

It must be noted that this effect is mostly attributed to a rise in the employment probability (see e.g. [Rebollo-Sanz & Rodríguez-Planas 2020](#)). However, according to [Reize \(2000\)](#), the destination state does not necessarily correspond to employment. The self-employment probability could rise as well. If self-employment mostly depends on ability rather than search efforts for market opportunities and the relative value of employment declines as the reservation wage decreases, we could also investigate whether the **UI** benefit cut may lead to more *opportunity-driven* entrepreneurs. This is not clear ex-ante and needs to be empirically analyzed.

In line with Standard Search Theory, one could also think of a different scenario. As the reservation wage for employment decreases and potential employees become cheaper, this could lead to an increase in vacancies in the general equilibrium. In this situation we would expect a higher job-finding rate, whereas self-employment may be rather unaffected (or in relative terms less likely to occur). It is unclear whether this could affect the quality of self-employment, i.e. whether the cut in benefits leads to more or less *necessity* entrepreneurship.

**Entrepreneurial Choice Model Hypothesis.** Regarding the extended version of the Entrepreneurial Choice Model in the spirit of [Alba-Ramirez \(1994\)](#), the decrease in benefits shortens actual UI duration. This may lead to less human capital deterioration and relatively better employment prospects compared to a setting without changes in UI benefits. As the reservation wage remains on a higher level, the expected income from employment would decrease relatively less. Simultaneously, a shorter UI benefit duration implies a shorter period of learning regarding market opportunities. The entrepreneurial ability improves relatively less, which leads to worse self-employment prospects. Thus, the Entrepreneurial Choice Model predicts a decrease in the self-employment probability.

### 2.2.3 Determinants of the Self-Employment Probability

It is not yet clear what kind of mechanisms can explain the transition from unemployment towards self-employment. Standard Search Theory analyzes unemployment duration and it is used to model the transition into employment. Most authors ignore that a transition from unemployment into self-employment is also possible. Conversely, the Entrepreneurial Choice Model investigates the entry decision into self-employment, ignoring a possible prior state of unemployment. Nonetheless, [Reize \(2000\)](#) suggests that the basic determinants of self-employment, “*e.g. expected income, risk aversion, human capital, assets, IRR<sup>11</sup>, social background, etc.*”, should be inferred from both theories.

To the best of our knowledge, only [Carrasco \(1999\)](#) analyzes elements of both Standard Search Theory and Entrepreneurial Choice Model in the case of Spain. Previously employed and unemployed individuals are considered separately. The author estimates the self-employment probability of previously employed individuals in a binomial logit framework, where remaining employed constitutes the reference group.<sup>12</sup> Regarding previously unemployed individuals, [Carrasco \(1999\)](#) estimates a multinomial logit model. She differentiates between self-employment and employment as possible destinations, while remaining unemployed constitutes the reference group. The author distinguishes unemployed individuals who receive UI benefits from those unemployed but ineligible to benefits – two basic elements from Standard Search Theory. She finds that benefit recipients are less likely to become self-employed (disincentive effect) compared to individuals who are unemployed but ineligible to benefits (entitlement effect). However, she connects these findings to the Disadvantage Theory.<sup>13</sup> According to [Reize \(2000\)](#), this result may also point towards risk aversion, a basic element of the Entrepreneurial Choice Model.<sup>14</sup>

<sup>11</sup>The income replacement ratio (IRR) is the ratio of unemployment benefits to the expected (self-)employment income ([Reize 2000](#)).

<sup>12</sup>Additionally, the author estimates a multinomial logit model, distinguishing the destinations of being self-employed without employees, self-employed with employees, and remaining employed ([Carrasco 1999](#)).

<sup>13</sup>This theory predicts that labor markets are *misfit* for those who are ineligible for benefits (they could not contribute long enough to be entitled for benefits) because they cannot obtain enough labor market experience or skills. Potential employers may consider them as low-quality workers. Thus, these workers are pushed into self-employment.

<sup>14</sup>As self-employment income is more uncertain than UI benefits, risk-averse UI recipients will only switch into self-employment if their expected income considerably exceeds their UI benefits. Conversely, unemployed individuals ineligible for benefits have “nothing to lose” and are more likely to switch into self-employment ([Reize 2000](#)).

## 2.3 Data and Descriptive Analysis

Before investigating the hypotheses explained in the previous section, we first explain the administrative data used for studying our research question and the construction of our analysis dataset. Second, we demonstrate the success of our data-building process by illustrating to which extent we are able to match relevant labor market stocks and flows over time with official statistics for Spain. In this context, we provide a descriptive analysis of the Spanish labor market, in particular with respect to analyzing transitions between unemployment, self-employment and employment over time (2005-2017) including the whole economic crisis period. Therefore, this section illustrates the relevance of our research questions, establishes interesting facts that may be of interest for future research and provides some insights into how the data can be used.

### 2.3.1 MCVL Data

The dataset used for the analysis is Spain's **MCVL**. It contains administrative information on individual socioeconomic characteristics and longitudinal information on labor market statuses and job characteristics for a four percent non-stratified random sample of Spain's population. The **MCVL** takes into account individuals who have been registered for social security at any point since 2005 until 2017, but it also entails reliable employment histories retrospectively since the 1980s (cf. Appendix II.3.1). It has been released in 14 waves, from the **MCVL** 2005 wave until the 2017 wave. As the anonymized identifiers are maintained, all **MCVL** editions can be combined. This leads to a high representativeness of the data and, as opposed to survey data, there is no problem concerning sample attrition using **MCVL** data. More detailed information on the **MCVL** and how we constructed our analysis dataset from the original raw data is provided in Appendix II.3.

**MCVL** data identifies five different labor market spells: 1) employment; 2) self-employment; 3) receiving **UI** benefits; 4) receiving **unemployment assistance (UA)** benefits; and 5) inactivity. The retrospective nature of the data enables to track an individual over his or her whole labor market history. Starting from the point when an individual joined a social security scheme for the first time, the labor market trajectory can be tracked until 2017. Naturally, the forthcoming spells after 2017 are right-censored with the exception of individuals who deceased earlier. The spells 1) to 4) connote that the individuals are actively registered with the social security authorities, whereas individuals in spell 5) are unregistered. Aside from individual labor market trajectories, **MCVL** data contains job characteristics. For each employment spell, it provides information on sector, occupation, skill level required for this job, contract type (fixed-term vs. open-end contract, as well as part-time vs. full-time contract), contribution basis, reason of dismissal, firm ownership (private vs. public), and the firm's location. As an individual's spell entry/exit date can be observed, (self-)employment experience can be computed.<sup>15</sup>

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<sup>15</sup>Following the definition of [De La Roca & Puga \(2017\)](#), we computed experience as accumulated time spent in employment, starting from the first job in an individual's life.

The socioeconomic characteristics entail an individual's age, sex, date of birth/death, country of birth, nationality, and formal education. The current province where the individual resides by the time he or she gets unemployed can be inferred from the province code where the individual is registered as **UI** recipient (compare Appendix II.3.3).

**Restrictions.** In general, for the construction of the quarterly dataset which we use to obtain the relevant descriptive statistics, we restrict our sample to the individuals of working age (18 years or older), who are included in the social security files from 2005 to 2017. However, some additional restrictions are necessary to carry out the different estimations. The analysis dataset for the **DiD** approach is restricted to individuals who enter their **UI** benefit spell between 1 January 2011 and 31 December 2013 (large sample). In addition to that, we restrict our sample to individuals with at least 120 days of **UI** benefit entitlement duration. Regarding the **RDD**, the analysis dataset only includes individuals who enter their **UI** benefit spell between 1 July 2011 and 31 July 2013 (medium sample) and with an **UI** entitlement duration of at least 180 days.

Moreover, it is important to note that some individuals have multiple **UI** benefit spells within the respective period of interest. For those cases, we only keep one random draw instead of pooling **UI** spells and treating them as if they stem from different individuals. Furthermore, we exclude individuals who turn older than 52 years within our analysis period, and use minimum age restrictions of 20 and 35 as a robustness check. In contrast to other authors (e.g. De La Roca & Puga 2017, Rebollo-Sanz & Rodríguez-Planas 2020), we do not exclude formerly self-employed individuals or those who worked in the agricultural sector before getting unemployed. We also include individuals who worked part-time in their pre-displacement job. As described in Appendix II.3.2, exact procedures to replicate our results and sample datasets can be inferred from our data documentations.

### 2.3.2 Other Data

While processing the data, the nominal contribution basis was deflated using the Consumer Price Index (CPI) with 2015 as a base year. Furthermore, some other macroeconomic indicators which are interesting for the data description and the analysis have been obtained from official sources. For instance, the quarterly real **Gross Domestic Product (GDP)** growth rate relative to the previous period is used as a control variable in our regressions. Similarly, the annual unemployment rate and some other labor market data, such as the self-employment rate or the labor force participation, have been extracted and used to generate the descriptive statistics which follow in Section 2.3.3. All of these indicators are drawn from the *Selected indicators for Spain* of the **OECD (2018)**<sup>16</sup> and the **INE (2018)**<sup>17</sup>.

<sup>16</sup>OECD data for Spain can be retrieved from: <https://data.oecd.org/spain.htm>.

<sup>17</sup>INE data for Spain can be retrieved from: <https://www.ine.es/>.

### 2.3.3 Descriptives - Matching Labor Market Flows

In the following, we document the main labor market statuses in Spain and describe how they evolved in the time period 2005-2017, thereby confirming our accuracy in constructing the dataset by showing that we are able to match key labor market facts as provided by official bodies like the [OECD](#) or the Spanish National Statistics Institution ([Instituto Nacional de Estadística \(INE\)](#)).

**Labor Force.** The composition of the labor force is plotted in [Figure II-1](#). The largest part of the labor force is compounded by employed workers. At the beginning of the sample period this part is almost constant at 78% and declines due to the crisis from 2008 onward, until a share of approximately 70% is reached. This drop of 8 percentage points (p.p.) is absorbed in the unemployed individuals' share which increases after the crisis by an equivalent amount. In contrast, the share of self-employed individuals stays roughly constant at 18%. A slight increase in the self-employment share is observable since 2013. Looking at the age distribution of the labor force, [Figure II-2](#) reveals that self-employment is more relevant for the older individuals (age groups over 40) than for younger individuals. The share of self-employed as percentage of the labor force is only around 10-15% for those younger than 40, whereas it ranges between 20-24% for those in the age groups above 40. A closer look at Spain's labor force levels from [OECD](#) data reveals that a four percent sample should equal on average 913,000 individuals across the sample period ([OECD 2018](#)).<sup>18</sup>

**Evolution of Labor Market Statuses.** In [Figure II-4](#) Spain's annual unemployment rate using [MCVL](#) and [OECD](#) data is illustrated for the sample period. The unemployment rates from both sources are based on individuals of working age and include all sectors, as well as all social security schemes, such that they are comparable. It is important to note that the [OECD](#) restricts the working age population to individuals between 15 and 64 years old, whilst the [INE](#)'s Working Conditions Survey (*Encuesta Nacional de Condiciones de Trabajo*) focuses on individuals who are 16 years or older. We restrict our descriptive sample to individuals who are 18 years or older<sup>19</sup>. In spite of these differences, the computed unemployment rate using [MCVL](#) data is very similar to the quarterly unemployment rate reported by [INE](#) (left panel figure) and also matches [OECD](#)'s annual unemployment rate (right panel figure). Concerning the self-employment rate that is measured with respect to the total employment rate, [Figure II-3](#) reconfirms that our data cleaning process and the construction of our dataset from the [MCVL](#) data enable us to match quarterly statistics from [INE](#) (left-hand panel), as well as annual statistics from [OECD](#) data (right-hand panel). Specifically, [Figure II-3](#) in the Appendix shows that self-employment has been slowly rising until reaching a peak in 2014 at nearly 20% and then declining again.

<sup>18</sup>The Spanish average labor force level from 2005 until 2015 was about 22,817,000 individuals per year ([OECD 2018](#)). Thus, a four percent sample should result in  $0.04 \cdot 22,817,000 \approx 913,000$  individuals.

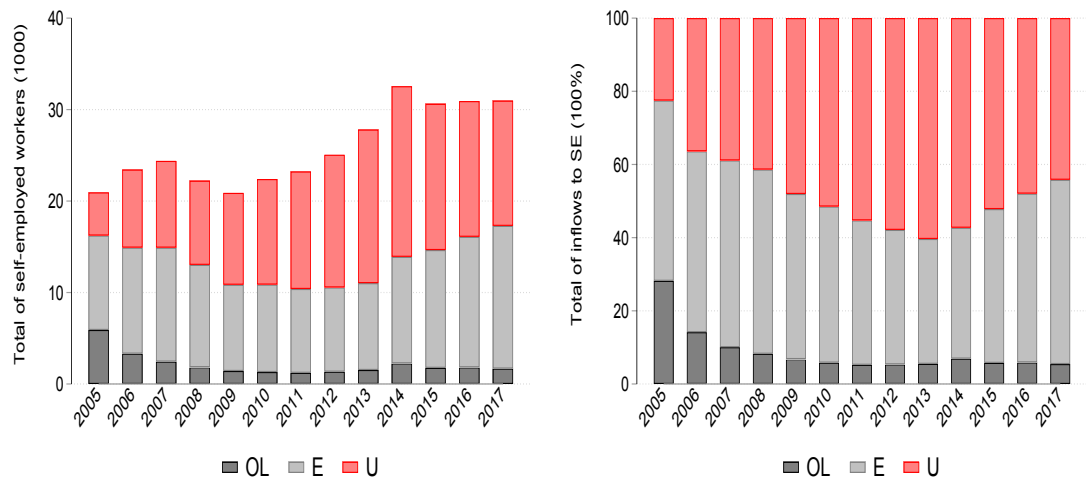
<sup>19</sup>For a summary of the main characteristics of the sample, on a spell basis, see Appendix [Table II.1](#).



For completeness, [Figure II-5](#) illustrates part-time and [Figure II-6](#) temporary contract rates in Spain. Again, note that we compare our calculated data with both official statistics from Spain (quarterly [INE](#) data in the left-hand panels) and [OECD](#) data (annual data in the right-hand panels). In particular, while the part-time rate has increased from around 10% to 15% during the whole sample period, the temporary contract rate reflects a U-shape evolution. This is in line with the observation that during an economic crisis temporary contracts are not renewed, and therefore this group of workers is among the first to be laid-off (as can be seen from the drop of around 30 to 23% during the crisis). In contrast, when the recovery started (in Spain around the end of 2013) temporary employment recovered first and in 2017 reached pre-crisis levels.

**Labor Market Flows.** [Figure 2-1](#) shows that the inflow into self-employment is considerably dominated by flows from unemployment. Thus, a relevant share of founders in Spain has been previously unemployed, and the inflow from unemployment into self-employment is important. It makes up 30-50% of all new self-employed every year.<sup>20</sup> Moreover, the evolution of the composition of inflows into self-employment exhibits counter-cyclical patterns, especially from 2010 onward. The share of inflows from previously employed workers decreases, the one of previously unemployed individuals increases during a crisis. Although it is true that the outflows from unemployment to self-employment may only reflect 5% of the whole unemployment stock ([Figure II-12](#)), it should be noted that usually there exist job spillovers, i.e. most founders have employees.

**Figure 2-1: Composition of Inflows into Self-Employment excl. Stocks**



*Notes:* These figures illustrate the yearly inflows to self-employment in Spain, in both absolute (left-hand side) and relative (right-hand side) terms. The sample consists of all individuals older than 18. We distinguish inflows of individuals from the relevant states: out of the labor force (OL), dependent employment (E), and unemployment (U).  
*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data.

<sup>20</sup>In the Appendix, we show also the same figures including the stocks of self-employed. Looking at stocks of self-employed individuals in our representative sample shows that about 80% of the self-employed remain in self-employment (SE) in the next year (less during the crisis period): [Figure II-7](#) shows the yearly inflows including the SE stock dimension, [Figure II-8](#) shows the same for outflows from SE. The graphs confirm that new inflows into SE are mainly composed out of new self-employed individuals who were previously unemployed or employed. In particular, the share of new inflows to SE out of unemployment increases until around 2013.

Consequently, the economic significance of this flow is a multiple of the outflow statistics from unemployment to self-employment and quantitatively important. In other words, startups can be engines for economic growth. Note that the role of self-employment for the inflows into employment (Figure II-9) appears not to change much over time. This is also true for the outflows from employment to self-employment (Figure II-10), but different to the patterns when analyzing the outflows from unemployment. We observe in Figure II-12 that the share of individuals who transition from unemployment to self-employment remains quite stable during the years surrounding the reform, even though they are relatively larger than at the beginning of the sample period. However, the outflows from unemployment are clearly dominated by employment, especially during the years 2012 and 2013. Moreover, Figure II-11 shows a similar pattern regarding the inflows into unemployment. The relative destruction of employment increases year by year until 2012/2013, then the economic recovery leads to a decreasing trend thereafter, by which the inflow into unemployment from employment declines.

**Self-Employment Characteristics.** Table II.2 shows a comparison of the main characteristics of individuals in self-employment, compared to individuals in dependent employment. Regarding their socioeconomic features, we observe that there is a gender gap in the group of self-employed individuals: while 48% of the individuals in the sub-sample of dependent employment correspond to female workers, only 36% of individuals in the self-employment group are female. Moreover, the average age in the self-employment group is slightly larger than in the sub-sample of employees. We also notice how the distribution of education levels differs to some extent across groups: e.g. the share of highly educated workers is larger for self-employment than employment. This may be due to the fact that the service sector is more important for self-employment (39% of all SE spells).

Moreover, Figure II-13 illustrates the composition of self-employment with respect to the sector in which the business has been started. We can confirm the fact that self-employment is indeed quite important in the construction sector. Figure II-13 shows that the share of workers in that sector increases until 2008, when it starts to decrease in favor of other sectors like transport, tourism and retail, but also professional, scientific, administrative and auxiliary services. Lastly, we obtain information on the average amount of experience and tenure in both groups, as well as concerning the spell duration and daily earnings. In this context, it becomes apparent that the average duration of the self-employment spells is remarkably larger than that of the employment spells (Table II.2). The self-employed individuals tend to remain longer in their position than the average employee, which indicates that many of the self-employed are successful, and that a majority of them are rather *opportunity* than *necessity-driven* entrepreneurs.

**Earnings.** Figure II-16 shows yearly mean earnings comparing our income data from either tax or from social security data. The mean annual labor income is about 20,000 Euro. It declined during the crisis and is recovering again since 2014, but still below the



2007 pre-crisis level. [Figure II-17](#) shows that monthly earnings translate into about 1,700 Euro, which corresponds to 60 Euro on a daily basis. This helps to ease the interpretation of the size of Spanish [UI](#) benefit levels (relative to other countries). [Figure II-15](#) shows that the distribution of monthly earnings is skewed to the left with a large dispersion across top incomes, but that most Spanish citizens earn income that is below the mean.<sup>21</sup>

## 2.4 Institutional Framework and Reform

Spain's institutional framework provides contributory social security protection covering healthcare, professional care for illnesses or accidents, and benefits for (temporary) disability, maternity, paternity, family, death, retirement, and job loss. The basic needs within these areas are also covered by non-contributory assistance benefits. Contributory benefits always have priority over non-contributory benefits and eligible individuals must claim them first ([SEPE 2019](#)). In the following, we focus on two different types of unemployment benefits. For details on the institutional background, we refer to our [Appendix II.4](#).

### 2.4.1 Unemployment Benefits in Spain

**Unemployment Insurance (UI) Benefits.** In order to receive contributory [UI](#) benefits, an individual must be legally unemployed, between 16 and 65 years old, must have contributed for a minimum of 12 months within the last six years, and the reason of unemployment must be an involuntary dismissal ([SEPE 2019](#)). The monthly [UI](#) benefit amount is computed from the regulatory base, which is an approximation of the average labor income over the six months preceding the unemployment spell, multiplied by the replacement rate. For the first six months of the [UI](#) benefit period, a replacement rate of 70% is applied. If the individual is entitled to more than six months of [UI](#) benefits, another replacement rate is valid from day 181 onward. This second rate corresponded to 60% in the time period before the reform of July 2012 took place. This reform reduced the second replacement rate to 50% of the regulatory base.

According to the [SEPE \(2019\)](#), the monthly [UI](#) benefit amount is subject to a floor of 80% of the Public Income Index (*IPREM*)<sup>22</sup> and a ceiling of 225% of the *IPREM*. It is increased by one sixth of the monthly benefit amount conditional on the number of dependent children. Details on the calculation of [UI](#) benefits can be inferred from [Table II.3](#) in the Appendix. Moreover, the bounds of [UI](#) (and of unemployment assistance) benefit amounts were kept constant between 2010 and 2016, when the *IPREM* was frozen. In other words, during the period of this chapter's analysis, all relevant social security benefit levels were kept nominally constant in Spain.

<sup>21</sup>This observation is in line with the increase in earnings inequality during the recession which mostly affected the bottom half of the distribution ([Bonhomme & Hospido 2017](#)).

<sup>22</sup>The *IPREM* serves as a reference to calculate social security benefits. By virtue of section 1 of Royal Legislative Decree No. 3/2004 of 25 June 2004, the *IPREM* replaced the minimum wage which was previously used to calculate social benefit amounts. The *IPREM* is revised on an annual basis.

**Table 2.1:** Duration of Entitlement to UI Benefits

<b>Contribution Period</b> (in months)	<b>Benefit Period</b> (in months)
< 12	0
12-17	4
18-23	6
24-29	8
30-35	10
36-41	12
42-47	14
48-53	16
54-59	18
60-65	20
66-71	22
≥ 72	24

*Notes:* This table summarizes the Spanish system of **UI PBD**. Eligibility requires a minimum contribution period of 12 months. **PBD** ranges from 4 to 24 months, and it is a function of the individual's months of contribution.

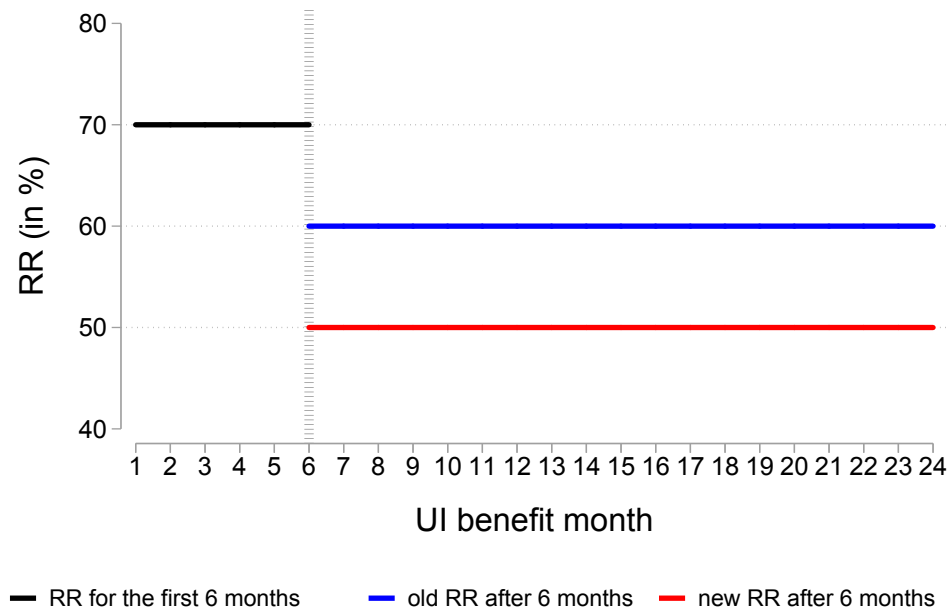
*Source:* Authors' own illustration based on the [SEPE \(2019\)](#).

The duration of entitlement to **UI** benefits depends on the contribution period. [Table 2.1](#) shows that the potential benefit duration (**PBD**) starts from a minimum of four months given a contribution period of at least one year. It increases step-wise by two months conditional on the respective length of the contribution period (first column in [Table 2.1](#)). The maximum possible **UI** benefit duration is 24 months ([SEPE 2019](#)). For more details on the Spanish **UI** system, we refer to Section [II.4.2](#) in Appendix [II.4](#).

**Unemployment Assistance (UA) Benefits.** Registered job seekers may be eligible for non-contributory **UA** benefits under certain circumstances. In case such a job searching individual is ineligible for **UI** benefits and if the monthly gross incomes correspond to at most 75% of Spain's minimum wage, he or she can claim **UA** benefits. Additional information on the **UA** system is provided in Section [II.4.3](#) of Appendix [II.4](#).

## **2.4.2 Labor Market Reform in 2012**

By virtue of the Real Decree-Law 20/2012 which aimed to ensure budgetary stability and competitiveness, details on the labor market reform discussed in this chapter were only publicly announced on 13 July 2012. On this day, Spain's vice president explained that all recipients entitled to more than six months of **UI** benefits who would start their **UI** spell on 15 July 2012 or thereafter would experience a reduced replacement rate of 50% after their first six months of receiving **UI** benefits. Thus, this reform decreased **UI** benefits by approximately 16.67% in comparison to the previous replacement rate of 60%. This new replacement rate is marked by the red line in [Figure 2.2](#). For all **UI** recipients who entered the **UI** system before 15 July 2012 the old rate (blue line) remained valid from day 181 of the benefit period onward. As illustrated by the black line, the replacement rate of 70% for the first six months of the **UI PBD** remained unchanged.

**Figure 2-2: Replacement Rate Before and After the Reform**

*Notes:* This figure illustrates the drop in the replacement rate of the UI benefits that took effect on 15 July 2012.

*Source:* Authors' illustration of the reform.

Rebollo-Sanz & Rodríguez-Planas (2020) note that the public was quickly aware of the reform's consequences regarding the UI benefit amount because the new law received medial attention and the government informed the people via several channels. Nonetheless, a displaced worker's decision to claim his or her benefits should not have been affected by the reform because for the first six months of benefit receipt the replacement rate stayed the same. Aside from that, as the reduction kicks in six months after the beginning of the benefit spell, it is possible to investigate the responses in individuals' job search behavior before and after the actual drop in the net replacement rate (i.e. in the UI benefit level) takes place.

According to Rebollo-Sanz & Rodríguez-Planas (2020), strategic lay-offs caused by the new law have been rather improbable because the reform was implemented already two days after its announcement. Moreover, they argue that trends of monthly inflows into the UI system have been similar during 2011 and 2012. As we discuss in Section 2.5.1, our analysis confirms that strategic manipulation around the reform cutoff date is not an issue, and thus the reform can be exploited as quasi-experiment. Moreover, the implementation of this reform affected a large share of the Spanish labor force, and could not be avoided in times when the economy was unlikely to improve for many months to come (Rebollo-Sanz & Rodríguez-Planas 2020). This argumentation is plausible because the unemployment rate reached its zenith of 26.1% in 2013 (OECD 2018).

Besides the reduction in the replacement rate, the new law also changed labor market rules for part-time workers and workers older than 52. For a detailed overview of all reforms, we refer to our Appendix Section II.4.6 in Appendix II.4.

## 2.5 Empirical Strategy

### 2.5.1 Difference-in-Differences (DiD) Approach

#### DiD Methodology

The goal of this chapter is to investigate the effect of a decrease in the **UI** benefit level on the self-employment probability in Spain. Such an **UI** benefit level change was implemented in the context of the labor market reform agenda 2012. The new law lowered the replacement rate after the first six months of an individual's **UI** benefit receipt by about 16.67% (see [Section 2.4.2](#)). As this reform affected only individuals entitled to more than six months of benefits who entered their **UI** benefit spell after 15 July 2012, one can differentiate between treated and untreated individuals who entered their **UI** benefit spell either during the pre- or during the post-reform period.

We exploit this quasi-experimental setup to identify the causal reform effect of this **UI** benefit level reduction using a parametric Difference-in-Differences (**DiD**) approach. While [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#) estimate the reform effect on the job-finding probability in employment, we apply the **DiD** strategy additionally to self-employment and total employment (= employment + self-employment). In other words, we decompose the total employment effect of the reform into the effect on self-employment and employment (vs. staying unemployed). Consequently, we are able to identify the potential bias that emerges through ignoring self-employment and only focusing on employment. We implement this strategy estimating a linear probability model separately for each month<sup>23</sup> after **UI** spell entry as illustrated below:

$$P(Y_{it} = 1 | T_i, POST_i, X_{ij}) = \alpha + \beta \cdot T_i + \gamma \cdot POST_i + \delta \cdot T_i \times POST_i + \sum_{j=1}^J \lambda_j \cdot X_{ij} + \varepsilon_{it} \quad (2.1)$$

Individuals who entered into unemployment prior to 15 July (1 January 2011 - 14 July 2012) are assigned to the pre-reform period, whereas those who entered into unemployment on 15 July or later (15 July 2012 - 31 December 2013) are assigned to the post-reform period. This circumstance is indicated by the binary variable  $POST_i$  which takes either the value 0 when an individual belongs to the pre-reform period ( $POST_i = 0$ ), or the value 1 when belonging to the post-reform period group ( $POST_i = 1$ ). Moreover, individuals are assigned to either treatment or control group. Those entitled to more than six months of **UI** benefits constitute the treatment group which is represented by the dummy variable,  $T_i$ , taking the value 1 ( $T_i = 1$ ). Conversely, individuals entitled to **UI** benefits of not more than six months form the control group ( $T_i = 0$ ).

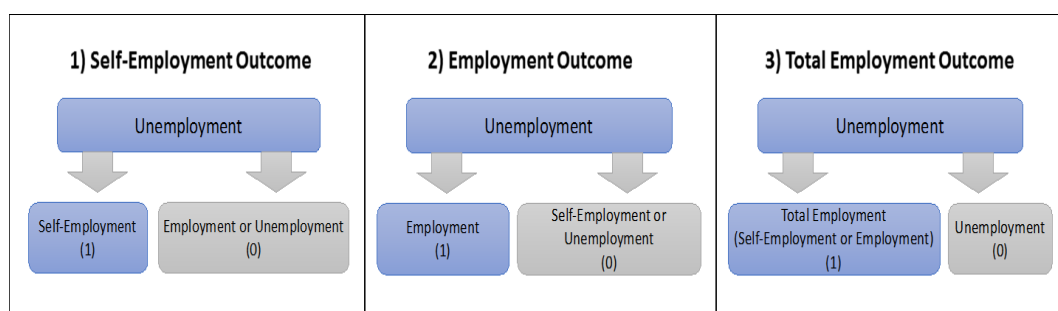
<sup>23</sup>We measure months approximately by taking four-week intervals.

Our estimation sample includes individuals who received **UI** benefits within the period between 1 January 2011 and 31 December 2013 (large sample). We follow them over their unemployment<sup>24</sup> spell until they exit into a new job, become self-employed or their observations get censored. Furthermore, the sample is restricted to individuals of age 35-52 with an **UI** entitlement length of at least 120 days to align treatment and control groups. In addition, we apply the same strategy to a sample with an age restriction that is in line with [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#), i.e. with individuals who are 35 to 52 years old.

Three different sets of outcome variables are used. In the first set, our dependent variable  $Y_{it}$  represents a binary outcome that takes the value 1 if individual  $i$  exits from unemployment into the state of interest (either self-employment, employment or total employment - see [Figure 2.3](#)) in month  $t$ , given that we still observe this individual as being unemployed at the beginning of month  $t$ . The outcome variable takes the value 0 if the individual stays unemployed in month  $t$ . Consequently, the effects on three different outcome variables per month  $t$  can be estimated. We follow each individual's **UI** spell over 26 months and identify the dynamic development of the **Average Treatment Effect (ATE)**. The second set of outcome variables measures whether the individual became self-employed or employed *within a certain amount of months*  $t$ . We chose intervals of 6, 12, 18, and 24 months. Finally, a set of unemployment duration variables is used as outcome variable to estimate different types of duration elasticities. In particular, we distinguish between the total unemployment spell duration (considering **UI**, **UA** and unregistered periods as job seeker to constitute unemployment) and the **UI** spell duration.

In our basic setting, only models including the group dummy  $T_i$ ,  $POST_i$  and their interaction are estimated. In further steps, different sets of control variables are added (represented by vector  $X_{ij}$ ). All of them are measured at an individual's **UI** spell entry. Socioeconomic variables include a female dummy, log of age, educational level dummies (lower, secondary, and university education), an indicator whether the individual has

**Figure 2-3:** Illustration of Binary Outcome Variables



*Notes:* Besides **UI** spells, unemployment also includes **UA** spells and unregistered spells which we count as being unemployed without receiving any kind of benefits. For more details, see also [Appendix II.3.3](#).

*Source:* Authors' own illustration.

<sup>24</sup>The unemployment spell includes both **UI** and **UA** receipt and counts unregistered periods as unemployment spells without receiving any kind of benefits.

children, and an immigrant dummy. Macroeconomic control variables include quarterly real GDP growth rate, month indicators, and dummy variables for all Spanish Autonomous Regions. Ultimately, the vector of controls entails a set of pre-displacement job characteristics: log of employment experience, log of self-employment experience, and occupational skill level (high, medium, low skilled). The summary statistics of all variables used in this chapter are presented in Tables II.4 and II.5 of the Appendix. The variables' exact definitions can be inferred from Appendix II.3.3.

### DiD Identification

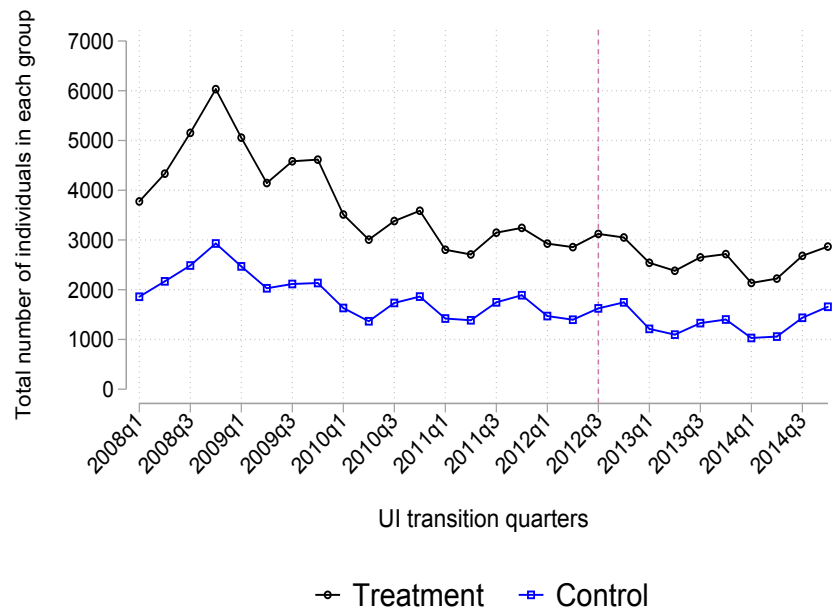
The identification strategy of the reform effect can be summarized in the following steps: first, estimating the (self-)employment probabilities of treated relative to non-treated individuals; and second, comparing both groups across time, i.e. those who were displaced in the post-reform period with those displaced in the pre-reform period. The additional comparison with workers displaced at the same time but assigned to the control group (CG) is used to cancel out other factors that may have systematically affected both groups.

Unlike in a laboratory experiment, we only observe individuals in one of the four states (in the pre-reform or post-reform period, and belonging to the control or treatment group). So first of all, our identification strategy requires that the composition of treatment and control group is not affected by the reform itself. Additionally, our identification strategy requires that the treatment and control group behave similarly regarding our outcome variable in the pre-reform period. In other words, the DiD estimator can only be unbiased if the *parallel-trend assumption* holds. Then, the average of the control group captures the counterfactual development of the treatment group and we can identify the causal reform effect. As long as their composition stays constant and time shocks are common to both groups, the *parallel trend assumption* holds. In this case, treatment and control group are allowed to start at different levels of the outcome variable.

Figure 2.4 illustrates the quarterly number<sup>25</sup> of UI inflows by group. The quarterly inflow level is constantly higher with regards to the treatment group compared to the control group. However, the composition of both groups' inflows seems to develop fairly parallel, which could serve as evidence in favor of a fixed group composition. Moreover, there is no evidence of UI entry date manipulation, i.e. there are no suspicious spikes right before the reform was implemented (red line). This finding speaks in favor of a fixed group composition and emphasizes the statement of Rebollo-Sanz & Rodríguez-Planas (2020) that strategic lay-offs to avoid the replacement rate reduction are rather unlikely since the reform was implemented already two days after its announcement.

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<sup>25</sup>In the Appendix, Figure II-18 shows the corresponding figure in percentage terms or Figure II-19 in a monthly dimension.

**Figure 2-4: UI Transitions (Total Numbers)**

*Notes:* This figure illustrates the quarterly transitions into UI, i.e. the total number of individuals in both the treatment and the control group who switch into UI in each quarter. The sample is restricted to individuals who are 20 to 52 years old, with an UI benefit entitlement length of at least 120 days, whose transition takes place between the first quarter of 2008 and the last quarter of 2014. The reform quarter is highlighted with a red dashed line. In the Appendix, [Figure II-18](#) shows the corresponding figure in percentage terms or [Figure II-19](#) in a monthly dimension.

*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data.

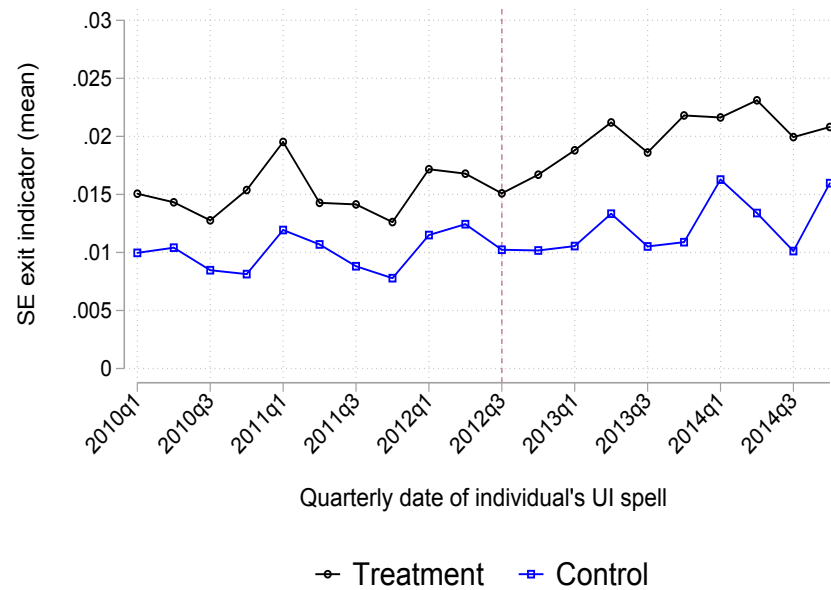
[Figure 2-5](#) illustrates the validity of the *parallel trend assumption*. It plots quarterly average probabilities of exiting into self-employment<sup>26</sup> in the period between Q1/2010 and Q4/2014. The sample is restricted to individuals of age 20-52, with an UI entitlement length of at least 120 days; and the reform quarter is highlighted in red. This figure shows that the common trend assumption holds, i.e. that treatment and control group seem to have parallel trends with respect to their outcome variables before the reform took place.

Even though our evidence speaks in favor of both a fixed group composition and a valid common trend assumption, some risk remains that the DiD estimator is biased due to inherent differences between the groups. [Tables II.6](#) and [II.7](#) show mean comparison tests of some interesting covariates between the two groups for different age restrictions. Indeed, most of the variables are significantly different between treatment and control groups, and some of them are included into our model (through  $X_{ij}$ ) to control for group differences to make parallel trends more plausible. Moreover, the conditional independence assumption<sup>27</sup> requires a full common support of both treated and non-treated individuals' characteristics. Thus, we decided to pre-select our sample based on the propensity score, under the awareness that implementing such trimming may come at the expense of some external validity, since the focus is set on a subset of the original sample.

<sup>26</sup>The parallel trend checks for employment and total employment as outcome variables can be inferred from Appendix [Figure II-20](#). They are in line with the findings with regards to self-employment.

<sup>27</sup>This asserts that treatment and control groups are, on average, comparable given some specific control variables.



**Figure 2-5: Parallel Trends Check for Self-Employment**

*Notes:* This figure illustrates quarterly average probabilities of exiting into self-employment in the period between the first quarter of 2010 and the last quarter of 2014. The sample is restricted to individuals who are 20 to 52 years old, with an UI entitlement length of at least 120 days. The reform quarter is highlighted with a red dashed line.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

In line with Crump et al. (2009), we only include individuals with a treatment propensity score between 0.1 and 0.9. Figures 2-4 and 2-5 and Tables II.6 and II.7 are already based on the pre-selected sample. However note, that without pscore trimming these figures look almost identical in shape.<sup>28</sup> The mean comparison tests improved slightly through pscore trimming and some of the differences turned insignificant or smaller in its magnitude.<sup>29</sup> The DiD analysis which follows in Section 2.6.1 is based on the pscore trimmed sample. Since the new law from 2012 also changed labor market rules for workers older than 52, this seems to be a reasonable maximum age restriction to avoid that other sections of the reform bias our results. In our main settings, we restrict our estimation sample even further to individuals who are between 35 and 52 years old. The reason for this is the Royal Decree-Law 4/2013, which was adopted on 22 February with the goal of promoting self-employment among young workers (defined as men younger than 30 and women younger than 35), and which could affect our results as well.

To sum up, the DiD approach allows us to estimate the average treatment effects of the reform in our quasi-experimental scenario. However, we can also take advantage of the discontinuity in the replacement rate which was the object of the 2012 labor market reform. In fact, focusing only on treated individuals, we would be able to identify local average treatment effects only for the respective group, providing us with a more complete picture for this impact evaluation.

<sup>28</sup>Figures without pscore trimming can be provided upon request. Note, that the levels in Figure 2-4 decrease as individuals with the lowest and highest pscore percentiles are excluded.

<sup>29</sup>Mean comparison tables without pscore trimming are available upon request.



## 2.5.2 Regression Discontinuity Design (RDD) Approach

### RDD Methodology

Besides the DiD approach, we also exploit the sharp discontinuity introduced by the reform using a Regression Discontinuity Design (RDD). Being affected by the reform is a deterministic and discontinuous function of time. We normalize the UI entry date of individual  $i$  to 0 at cutoff date (15 July 2012). Moreover, we only consider individuals with at least 180 days of UI benefit entitlement. In other words, we restrict the sample to the treatment group of our DiD analysis. In the RDD analysis, the term *control group* solely refers to individuals eligible to more than 180 days of UI benefits who entered their UI spell *before* the cutoff date. Consequently, individuals are *treated* if they entered their UI benefit spell after the cutoff date. Our estimation equation can be illustrated as follows:

$$Y_i = \alpha \cdot \mathbf{1}(t_i \geq 0) + \sum_{k=0}^K \gamma_{Ck} g(d_{ik}) \cdot \mathbf{1}(t_i \leq 0) + \sum_{k=0}^K \gamma_{Tk} g(d_{ik}) \cdot \mathbf{1}(t_i \geq 0) + \sum_{j=1}^J \lambda_j \cdot X_{ij} + \varepsilon_i \quad (2.2)$$

We use our set of *within* measures and UI/UE duration as outcome variables,  $Y_i$ . The vector of control variables,  $X_{ij}$ , contains the same set of variables as explained in the previous Section 2.5.1. The normalized UI entry date of individual  $i$  is represented by  $t_i$ , while  $d_{ik}$  measures the entry date's distance (in weeks  $k$ ) from the cutoff date. Polynomial  $g(\cdot)$  connects  $d_{ik}$  and  $Y_i$  when the UI entry date is below ( $t_i \leq 0$ ) or above ( $t_i \geq 0$ ) the cutoff date, respectively. Regarding  $g(\cdot)$ , we first worked with a local linear regression. However, the quadratic version of the polynomial provides a better fit to the data, and thus the main RDD results are based on the quadratic version of Equation (2.2). The coefficients  $\gamma_{Ck}$  and  $\gamma_{Tk}$  measure how the particular group assignment (treatment/control) affects the outcome variable, thereby taking into account potential group-specific UI entry date effects.

In the following, we focus on  $\alpha$  as our explanatory variable of interest. Instead of measuring an *average treatment effect (ATE)* as we did in our DiD analysis,  $\alpha$  now measures the *Average Treatment Effect on the Treated (ATET)* of a 10 p.p. drop in the replacement rate after 180 days of UI receipt for workers who switch into an UI spell in the vicinity of the cutoff date.

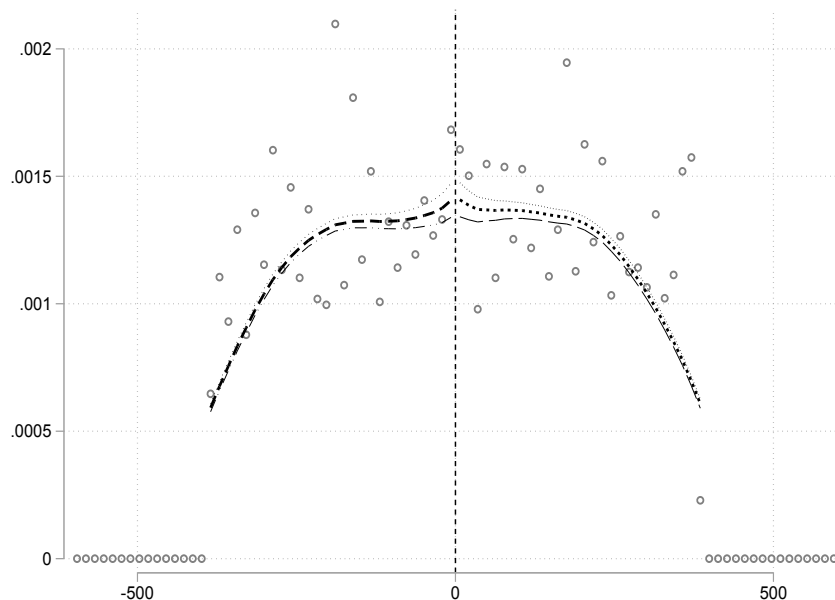
### RDD Identification

As assignment into treatment is solely determined by each individual's UI entry date, our RDD identification strategy of the causal ATET hinges crucially on the assumption that individuals cannot manipulate this running variable around the cutoff. Thus, we need continuity around the cutoff which can be analyzed by conducting a McCrary test with the null hypothesis of continuity.

Figure 2-6 shows the results of running the aforementioned test using our medium sample with a bandwidth of one year. This includes individuals who entered their UI spell between 1 July 2011 and 31 July 2013. According to the estimated test statistic of -0.0035 (0.0368), the null hypothesis cannot be rejected which confirms that our identification assumption holds. Details about the distribution of observations per bin<sup>30</sup> for the McCrary test can be inferred from the Appendix Figure II-21.

The reason for choosing the medium sample rather than the large sample (used in our DiD approach) is that individuals should be more comparable in a shorter time window, as they experience the same economic conditions. Nonetheless, we conducted our regression analysis with all three sample sizes (large, medium, and small<sup>31</sup>). The small and large sample McCrary test statistics also speak in favor of the existence of continuity around the cutoff date. Details can be inferred from the Appendix Figure II-22.

**Figure 2-6:** McCrary Test for Individuals Entering their UI Spell (medium sample)



*Notes:* This figure illustrates the McCrary test for individuals entering their UI spell within 380 days (medium sample) distance from the cutoff. The medium sample consists of individuals entering between 1 July 2011 and 31 July 2013. We consider individuals who are 35 to 52 years old. The discontinuity estimate (log difference in height) for the medium sample is -0.0035 (0.0368). Standard errors are shown in parentheses.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

<sup>30</sup>Bins are defined fortnightly.

<sup>31</sup>The small sample includes individuals who enter their UI benefit spell in 2012.

## 2.6 Results

### 2.6.1 DiD Results

#### DiD Main Specification Results

The results of the described DiD model are illustrated in Table 2.2 below. They show the reform's effect on the set of outcome variables that measure whether an individual transitions into employment, self-employment or total employment (= self-employment + employment) *within* a particular period of months (6, 12, 18, 24). In the following, we will refer to them as our *within measures*. The first column of each table shows the results of our general setting which includes 20-52 year old individuals, while our main results in the second column are based on individuals who are aged between 35 and 52 years to avoid potential bias from other reforms.<sup>32</sup> The estimates in Table 2.2 refer to the main model with all covariates as described in Section 2.5.1.

**Table 2.2:** Difference-in-Differences Main Results

(a) Self-Employment and Employment			(b) Total Employment and UI/UE Duration		
Outcome	20-52	35-52	Outcome	20-52	35-52
<i>Self-Employment</i>			<i>Total Employment</i>		
6 months	0.006** (0.002)	0.008 (0.005)	6 months	0.033*** (0.007)	0.029** (0.011)
12 months	0.007*** (0.002)	0.006 (0.006)	12 months	0.004 (0.009)	-0.002 (0.009)
18 months	0.005* (0.003)	0.006 (0.007)	18 months	-0.001 (0.007)	-0.020* (0.011)
24 months	0.005 (0.003)	0.006 (0.007)	24 months	-0.003 (0.006)	-0.022** (0.010)
<i>Employment</i>			<i>Unemployment Duration</i>		
6 months	0.027*** (0.007)	0.021* (0.011)	UI	-18.551*** (2.470)	-14.536*** (4.667)
12 months	-0.003 (0.010)	-0.008 (0.012)	UE	-9.549 (6.049)	-13.415 (9.319)
18 months	-0.006 (0.008)	-0.026** (0.011)			
24 months	-0.008 (0.007)	-0.029** (0.010)			
<i>N</i>	51,903	20,585	<i>N</i>	51,903	20,585

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the ATE coefficients but control for all covariates using our main specification. Our estimation sample includes individuals who switched into an UI spell between January 2011 and December 2013 (large sample), with an UI entitlement duration of at least 120 days.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

<sup>32</sup>Our age restriction excludes particular age groups which have been targeted by other reforms that could potentially bias our results (e.g. the Royal Decree-Law 4/2013 with the aim of promoting self-employment among the youth). For details on other reforms, see Appendix II.4.6.

The first column of [Table 2.2](#) shows that the average probability to become self-employed (within 6, 12, and 18 months after entering [UI](#)) is estimated to be significantly positive. However, this effect is very small and close to zero. It turns insignificant if we consider the probability to get self-employed within 24 months.

The reform affects our *within measures* of the average probability to become re-employed significantly positive (at the 1% level) for the first 6-month interval. According to [Table 2.2](#), the probability of finding a job within the first six months of unemployment increases by 2.7% in anticipation of the 16.67% reduction in the replacement rate that would follow after the sixth month. This constitutes evidence in favor of a behavioral response effect, which is in line with the results of [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#). However, the average reform effect on re-employment becomes slightly negative for the medium term (12 months) and the long term (18 and 24 months), but then also becomes statistically insignificant.

The right-hand panel of [Table 2.2](#) shows how the reform effects on self-employment and employment add up and shape the impact on total employment (TE). The effects on TE roughly mirror the estimates on employment. We find a highly significant positive effect on the TE probability within the first six months because then the effects on both self-employment and employment point into the same direction. For the medium run, the reform's impact on TE is positive but insignificant – suggesting that then the positive impact on self-employment dominates. In the long run, the effect on TE is negative but insignificant – suggesting that then the negative effect on re-employment dominates.

The additional restriction to focus on individuals who are 35 to 52 years old does not change the estimated coefficients' direction. We find that estimates of the self-employment probability turn completely insignificant, pointing towards a null effect on startups. Considering the *within measures* for employment and total employment, the estimates regarding the first six months remain positive but their statistical significance decreases (to the 10% level), whereas the negative long-run average effects become more significant (reaching the 5% level) and larger in size.<sup>33</sup> Since the average reform effect on the self-employment probability seems to be insignificant, the impact on total employment appears to be mainly driven by the effect on the employment probability.

Finally, the last panel of [Table 2.2](#) presents the reform effects on actual [UI](#) duration and on general unemployment ([UE](#)) duration. On average, the replacement rate drop of approximately 16.67% decreased actual [UI](#) benefit duration by 15 (19) days in case of the 35-52 (20-52) year old individuals. The estimated effects on actual [UE](#) duration are smaller in absolute terms but statistically insignificant.

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<sup>33</sup>In the context of the Spanish labor market reforms in 2001 and 2006, [Rebollo-Sanz \(2012\)](#) also finds that [UI](#) benefits have a negative effect on employment duration.

### DiD Robustness Checks

**Table 2.3** adds different sets of covariates  $X_{ij}$  (socioeconomic characteristics, macro-economic controls, and pre-displacement job characteristics) to the baseline setting, using our *within outcome measures* as dependent variables. In what follows, we focus on individuals of age 35-52 to avoid potential bias due to other reforms. The last column shows our main specification including all sets of control variables (as in **Table 2.2**).

**Table 2.3:** Difference-in-Differences Robustness

Outcome	Baseline	+Socioeconomic	+Macroeconomic	+Job Charac.
<i>Self-employment</i>				
6 months	0.008 (0.006)	0.009 (0.006)	0.008 (0.006)	0.008 (0.005)
12 months	0.005 (0.007)	0.006 (0.007)	0.006 (0.007)	0.006 (0.006)
18 months	0.005 (0.008)	0.006 (0.007)	0.005 (0.007)	0.006 (0.007)
24 months	0.005 (0.008)	0.006 (0.008)	0.006 (0.007)	0.006 (0.007)
<i>Employment</i>				
6 months	0.021 (0.013)	0.021* (0.012)	0.020 (0.012)	0.021* (0.011)
12 months	-0.010 (0.015)	-0.009 (0.013)	-0.010 (0.013)	-0.008 (0.012)
18 months	-0.027* (0.014)	-0.026* (0.013)	-0.028** (0.013)	-0.026** (0.011)
24 months	-0.030** (0.013)	-0.030** (0.011)	-0.031** (0.011)	-0.029** (0.010)
<i>Total Employment</i>				
6 months	0.029** (0.012)	0.030** (0.012)	0.028** (0.011)	0.029** (0.011)
12 months	-0.005 (0.012)	-0.003 (0.011)	-0.004 (0.010)	-0.002 (0.009)
18 months	-0.022* (0.012)	-0.021* (0.011)	-0.022* (0.011)	-0.020* (0.011)
24 months	-0.025** (0.011)	-0.024** (0.011)	-0.025** (0.010)	-0.022** (0.010)
<i>Unemployment Dur.</i>				
UI	-15.164*** (4.645)	-15.313*** (4.646)	-15.130*** (4.472)	-14.536*** (4.667)
UE	-12.474 (10.256)	-12.818 (9.950)	-12.502 (9.663)	-13.415 (9.319)
N	20,585	20,585	20,585	20,585

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the **ATE** coefficients but control for all additional sets of covariates described in the column header. Our estimation sample includes individuals who are 35 to 52 years old, and switched into an **UI** spell between January 2011 and December 2013 (large sample). They have an **UI** entitlement duration of at least 120 days.

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

Directions and magnitudes of the average effects in the main specification are already visible in the baseline setting. Again, the effects on our *within measures* of the self-employment probability are slightly positive but insignificant over all specifications. The reform affects our *within measures* of the (total) employment probability positively in the short run but negatively in the medium and long run. While the statistical significance slightly changes across different specifications, the point estimates remain stable. Moreover, the highly significant average reform effect on the actual **UI** benefit duration is very robust. Starting from a decrease in actual **UI** duration of almost 15 days in the baseline setting, this number slightly increases when adding socioeconomic controls, but then declines again when additionally controlling for macro-economic variables and pre-displacement job characteristics. The effect on general actual unemployment (**UE**) duration remains slightly smaller but insignificant across all specifications.

As our estimated average treatment effects of a reduction in **UI** benefits on actual **UI** duration are highly significant across all specifications, we can calculate precise **UI** benefit level duration elasticities. Instead, the importance attached to our computed **UE** duration elasticities should stay within reason, since they are based upon mostly insignificant point estimates. An overview of our estimated elasticities for the 35-52 year old individuals with different sets of covariates can be inferred from **Table 2.4**. The first row in the upper panel shows **UI** duration elasticities for the total sample (based on **Table 2.3**). In the second row, the sample is restricted to individuals who either continue to receive **UI** benefits or get employed. Those entering self-employment are excluded.

**Table 2.4:** UI and UE Duration Elasticities

Outcome	Baseline	+ Socioec.	+ Macroec.	+ Job Charact.
<i>UI</i>				
Total	0.410***	0.414***	0.409***	0.393***
Employment	0.412***	0.417***	0.420***	0.401***
Self-Employment	0.262**	0.321**	0.309**	0.334**
<i>UE</i>				
Total	0.203	0.208	0.203	0.218
Employment	0.198	0.208	0.211	0.225
Self-Employment	0.199	0.239	0.229	0.280

*Notes:* Significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table presents the **UI** and **UE** duration elasticities, computed from the **DiD** estimation results. The elasticity  $\eta$  is derived according to the following formula:  $\eta = \frac{\Delta(\text{duration})}{\Delta(RR)}$ . In other words, we calculate the elasticity based on the percentage variation in **UI** or **UE** duration (with respect to the average duration before the reform) divided by the variation in the replacement rates due to the reform (approx. 16.67%). The sample includes all individuals who are 35 to 52 years old, with an **UI** entitlement duration of at least 120 days, and who became unemployed between January 2011 and December 2013 (large sample). The results for the *Total* row header are based on the total sample used in **Table 2.3**. The *Employment* header corresponds to our sample that excludes individuals who transition into self-employment (it keeps those who either stay unemployed or find a job). The *Self-Employment* header corresponds to our sample that excludes individuals who find a job after their unemployment spell (it keeps those who stay either unemployed or transition into self-employment).

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

The third row shows estimated duration elasticities when restricting the sample to individuals who either continue to receive **UI** benefits or become self-employed. Hence, individuals who find a job are excluded. The second panel shows the results for the same samples but with the general **UE** duration as outcome variable. Details on the point estimates and the average values of actual **UI/UE** duration used to calculate these duration elasticities can be inferred from Appendix [Tables II.8](#) and [II.9](#).

We find that **UI** duration elasticities for future self-employed individuals are roughly 10 percentage points smaller than those of future employed individuals. This pattern stays stable across all specifications; and given that estimates of future self-employed individuals' **UI** duration elasticity did not exist so far, this finding is of particular interest. According to our estimates, a 16.67% decrease in the replacement rate leads on average to a 5.67% ( $= 16.67\% \cdot 0.334$ ) decrease in the **UI** benefit spell duration, in case we only focus on individuals who become self-employed (or stay unemployed). When focusing on individuals who exit into employment, a 16.67% decrease in the replacement rate leads on average to a 6.68% ( $= 16.67\% \cdot 0.401$ ) decrease in the **UI** benefit spell duration. If we augment our sample and include all individuals, regardless of their **UI** exit state, the average effect on the **UI** benefit duration corresponds to 6.55% ( $= 16.67\% \cdot 0.393$ ). Consequently, the exclusion of self-employed workers from the sample biases the average effect on **UI** duration slightly upwards.

Moreover, we find that **UE** duration elasticities are almost half the size of **UI** duration elasticities when it comes to (total) employment but similar regarding self-employment. In the main specification, the **UE** duration elasticity of future self-employed individuals is even larger than the one of future employed workers, but remains insignificant. Altogether, we find fairly robust **UI/UE** duration elasticity results for all post-unemployment outcomes.

### DiD Placebo Tests

To evaluate the plausibility of the quasi-experimental identification strategy which allows a causal interpretation of lower **UI** benefits induced by the drop in the replacement rate on our outcome variables of interest, it is important to conduct placebo tests ([Bertrand et al. 2004](#)). Our **DiD** placebo test estimations are presented in [Table 2.5](#). The first column shows the estimated results of our main specification. In the second column we demonstrate our findings when we set the reform to take place at a fictive date in July 2011. Clearly, our estimates are insignificant and close to zero which provides evidence in favor of our identification strategy. The third column corresponds to a fictive reform date in April 2012. Then, our point estimates become slightly larger in absolute terms but remain insignificant. In summary, placebo tests confirm the reliability of our **DiD** estimation approach.



**Table 2.5:** Difference-in-Differences Placebo Tests

Outcomes	PLACEBO TESTS		
	(1)	(2)	(3)
<i>Self-Employment</i>			
6 months	0.008 (0.005)	0.002 (0.004)	0.009 (0.011)
12 months	0.006 (0.006)	0.001 (0.005)	0.017 (0.015)
18 months	0.006 (0.007)	0.001 (0.005)	0.021 (0.017)
24 months	0.006 (0.007)	0.001 (0.005)	0.019 (0.017)
<i>Employment</i>			
6 months	0.021* (0.011)	0.003 (0.019)	-0.023 (0.021)
12 months	-0.008 (0.012)	0.016 (0.017)	-0.025 (0.024)
18 months	-0.026** (0.011)	0.007 (0.013)	-0.026 (0.027)
24 months	-0.029** (0.010)	0.010 (0.012)	-0.029 (0.032)
<i>Total Employment</i>			
6 months	0.029* (0.011)	0.004 (0.017)	-0.014 (0.027)
12 months	-0.002 (0.009)	0.016 (0.016)	-0.008 (0.026)
18 months	-0.020* (0.011)	0.008 (0.012)	-0.005 (0.026)
24 months	-0.022** (0.010)	0.011 (0.011)	-0.011 (0.027)
<i>N</i>	20,585	30,832	19,710

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the ATE coefficients but control for all covariates using our main specification. Column (1) shows our main setting from Table 2.2. In column (2), we set the reform to take place at a fictive date on 1 July 2011 (bandwidth of 12 months). In column (3), the fictive reform date is April 2012. All samples only include individuals who are 35 to 52 years old, with an UI entitlement of more than 120 days.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

### DiD Dynamic Average Treatment Effects

Figure 2.7 shows our estimated coefficients of the average treatment effect (ATE) [solid line] and their 95% confidence intervals [dashed lines] when using alternative outcome variables which measure individual  $i$ 's binary exit state in each month  $t$ .<sup>34</sup>

Panel (a) shows that the estimated ATEs on self-employment are slightly positive in the first five months of the UI spell. Then, they become slightly negative but they flatten out with increasing  $t$ . These ATEs are not significantly different from zero which also becomes clear when considering the scale. Thus, there does not seem to be a significant effect with respect to the self-employment probability in response to UI benefit cuts.

<sup>34</sup>Note that in this case we aim to compare these groups over time while allowing treatment effects to vary across points in time (heterogeneous time effects). See de Chaisemartin & D'Haultfoeuille (2019) for a more complete analysis of this type of estimations.



**Figure 2-7: Dynamic Average Treatment Effects (ATE)**

**Notes:** These figures illustrate our estimated coefficients for the average treatment effects (solid line), as well as their 95% confidence intervals (dashed lines), regarding each relevant outcome: self-employment, dependent employment and total employment. The sample is restricted to individuals who are 35 to 52 years old, and became unemployed between January 2011 and December 2013 (large sample). They have at least 120 days of UI benefit entitlement. Vertical dashed lines visually delimit the corresponding six-month periods.

**Source:** Authors' calculations based on MCVL 2005-2017 data.

Considering the ATEs on employment, as illustrated in panel (b), there is evidence for a positive behavioral response effect four to six months after entering the UI spell. This indicates that affected individuals react to a future reduction in UI benefits by increasing their search efforts.<sup>35</sup> After the drop in UI benefits took place in the sixth month, the ATE plummets to a significantly estimated coefficient of approximately  $-0.02$ . Consequently, reducing the replacement rate by 10 percentage points decreases the probability of finding a job seven months after entering the UI system, on average by 2%. Moreover, we also estimate a significantly negative ATE of 1% on the employment probability after 12 months of UI benefit receipt. The ATEs converge to zero when  $t$  further increases. This result illustrates that the estimated negative long-run effects on our *within measures* from Tables 2.2 and 2.3 are driven by negative effects on the employment probability in the months shortly after the replacement rate drop, and after the first year of UI benefit receipt. This feature is visible in the dynamic ATE figures. Finally, panel (c) of Figure 2-7 illustrates that the pattern for total employment mirrors the one of employment.

<sup>35</sup>These findings are in line with those of Marinescu & Skandalis (2019) who conclude that unemployed individuals strongly increase their number of applications as benefit exhaustion approaches, and decrease it after benefits exhaust.

In summary, the reform appears not to have an effect on self-employment but increases an individual's (total) employment probability just before the drop in **UI** benefits occurs. Shortly after that, the reform leads to a significantly negative effect on the probability of finding a job, most likely because the unemployed individuals' search intensity decreases. In the long run, there is no significant effect on the (total) employment probability.

### DiD Subgroup Analysis

Since the identified reform effects could be heterogeneously driven by specific subgroups, **Table 2.6** presents estimation results of a subgroup analysis when we use our *within outcome measures* as dependent variables. Each panel represents different subgroups (age older than 45 vs. at most 45, female vs. male, permanent vs. fixed-term contract, public vs. private sector) that are compared.

Potential subgroup differences with respect to age can be inferred from the first panel. The significantly positive effect on the short-run (total) employment probability seems to be driven by younger individuals. In contrast to that, the positive effect on the long-run self-employment probability seems to be more prevalent for older workers.

The second panel compares average estimates for female individuals with those of male individuals. We find that the slightly positive average effect on self-employment is entirely driven by male individuals. Turning towards employment, male individuals experience a slightly more intensified negative effect in the long run as compared to their female counterparts. As positive effects on males' self-employment and negative effects on their employment probability cancel out each other, effects for men turn insignificant when we consider total employment. In the long run, the cut in **UI** benefits significantly lowered women's probability to return to any sort of employment (total employment). In contrast, men tend to have slightly higher probabilities to become self-employed in the medium and long run. The long-run self-employment probability is increased further when it comes to older men as compared to young men, most likely because they have better financial resources.

In the third panel, we distinguish individuals with a permanent contract from those with a fixed-term contract in their pre-displacement jobs. The significantly positive reform effect on the (total) employment probability of our main short-run specification (within 6 months) seems to be partially driven by individuals who had a temporary contract before entering the **UI** system. Those with previously permanent contracts seem to be more negatively affected in the medium and long run. Finally, we distinguish former public and private sector workers. We find a larger positive reform effect on the short-run (total) employment probability of former public compared to private sector workers.

Our findings suggest a heterogeneous reform effect with respect to age, gender and pre-displacement contract characteristics. The positive short-run effects on (total) employment are partially driven by younger individuals who previously worked under a temporary contract. This effect is even stronger for former public sector workers.

In the medium and long run, negative average treatment effects on (total) employment seem to be more prevalent for individuals who previously worked under a permanent contract. The slightly positive effects on the self-employment probability are driven by male individuals. In the long run, older individuals also contribute to a slightly positive average reform effect on the self-employment probability. Results of this subgroup analysis for the **UI/UE** duration elasticity as outcome variable are provided in Appendix Table II.10.

**Table 2.6:** Difference-in-Differences Subgroup Analysis

<b>Age</b>	SE > 45	SE ≤ 45	E > 45	E ≤ 45	TE > 45	TE ≤ 45
6 months	0.017 (0.012)	0.006 (0.005)	-0.004 (0.026)	0.031** (0.012)	0.012 (0.019)	0.037*** (0.012)
12 months	0.015 (0.013)	0.002 (0.006)	-0.022 (0.031)	-0.002 (0.013)	-0.006 (0.027)	0.000 (0.012)
18 months	0.018 (0.012)	0.001 (0.007)	-0.033 (0.034)	-0.023 (0.013)	-0.015 (0.030)	-0.021 (0.013)
24 months	0.022* (0.013)	0.001 (0.007)	-0.035 (0.029)	-0.027** (0.012)	-0.013 (0.024)	-0.026* (0.012)
N	5,723	14,862	5,723	14,862	5,723	14,862
<b>Female</b>	SE (FEM.)	SE (MALE)	E (FEM.)	E (MALE)	TE (FEM.)	TE (MALE)
6 months	0.006 (0.006)	0.011 (0.007)	0.027 (0.019)	0.010 (0.016)	0.033 (0.020)	0.021 (0.014)
12 months	-0.005 (0.009)	0.018** (0.007)	0.001 (0.013)	-0.022 (0.020)	-0.004 (0.012)	-0.004 (0.019)
18 months	-0.003 (0.009)	0.015** (0.007)	-0.024** (0.011)	-0.032* (0.018)	-0.027** (0.010)	-0.017 (0.018)
24 months	-0.006 (0.009)	0.019** (0.007)	-0.027* (0.015)	-0.036*** (0.012)	-0.033** (0.013)	-0.017 (0.013)
N	10,475	10,110	10,475	10,110	10,475	10,110
<b>Permanent</b>	SE (PERM.)	SE (TEMP.)	E (PERM.)	E (TEMP.)	TE (PERM.)	TE (TEMP.)
6 months	0.017 (0.017)	0.001 (0.006)	-0.004 (0.030)	0.051*** (0.012)	0.013 (0.032)	0.052*** (0.011)
12 months	0.014 (0.018)	-0.004 (0.007)	-0.041* (0.022)	0.024 (0.014)	-0.028* (0.015)	0.020 (0.012)
18 months	0.021 (0.019)	-0.007 (0.007)	-0.046* (0.024)	-0.001 (0.013)	-0.025 (0.018)	-0.009 (0.012)
24 months	0.021 (0.019)	-0.005 (0.008)	-0.045 (0.028)	-0.007 (0.012)	-0.024 (0.019)	-0.013 (0.010)
N	7,537	13,048	7,537	13,048	7,537	13,048
<b>Public</b>	SE (PUB.)	SE (PRIV.)	E (PUB.)	E (PRIV.)	TE (PUB.)	TE (PRIV.)
6 months	0.011 (0.009)	0.009 (0.007)	0.089*** (0.028)	0.013 (0.012)	0.100*** (0.032)	0.022* (0.012)
12 months	0.008 (0.009)	0.007 (0.008)	0.001 (0.037)	-0.005 (0.014)	0.009 (0.040)	0.002 (0.012)
18 months	0.000 (0.009)	0.008 (0.008)	-0.028 (0.033)	-0.022* (0.012)	-0.028 (0.034)	-0.014 (0.011)
24 months	0.004 (0.009)	0.008 (0.008)	-0.034 (0.035)	-0.025* (0.012)	-0.030 (0.037)	-0.016 (0.011)
N	2,186	17,825	2,186	17,825	2,186	17,825

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the **ATE** coefficients but control for all covariates using our main specification. The sample includes all individuals who are 35 to 52 years old, with **UI** entitlement larger than 120 days, and who became unemployed between January 2011 and December 2013 (large sample). *Source:* Authors' calculations based on **MCVL** 2005-2017 data.

## 2.6.2 RDD Results

### RDD Main Specification Results

As Figure 2-6 shows, the RDD identification assumption is plausible and, thus, our empirical strategy allows to identify the causal local average treatment effect of the cut in UI benefits on the probability to enter self-employment or (total) employment, as well as on the UI duration elasticity. Table 2.7 presents our estimated coefficients based on the medium sample and a quadratic RDD set up. Again, we show the results for individuals who are 20 to 52 years old in the first column, and results for individuals who are 35 to 52 years old in the second column.

The direction of the reform effects does not change across age restrictions. As compared to the insignificant ATEs that we obtain using our DiD strategy, the ATET estimated by the RDD points towards a negative reform effect on the probability to become self-employed. These effects turn slightly significant when only considering individuals who are 35 to 52 years old.

**Table 2.7:** Regression Discontinuity Design Main Results

(a) Sel-Employment and Employment			(b) Total Employment and UI/UE Duration		
Outcome	20-52	35-52	Outcome	20-52	35-52
<i>Self-Employment</i>			<i>Total Employment</i>		
6 months	-0.013 (0.009)	-0.024* (0.015)	6 months	0.015 (0.028)	0.040 (0.036)
12 months	-0.017 (0.011)	-0.028* (0.016)	12 months	0.014 (0.024)	0.038 (0.032)
18 months	-0.017 (0.012)	-0.029* (0.017)	18 months	0.013 (0.021)	0.025 (0.028)
24 months	-0.019 (0.012)	-0.033** (0.016)	24 months	0.017 (0.017)	0.030 (0.023)
<i>Employment</i>			<i>Unemployment Duration</i>		
6 months	0.028 (0.030)	0.064 (0.040)	UI	-15.213 (11.113)	-25.235 (15.991)
12 months	0.032 (0.027)	0.066* (0.037)	UE	-27.310* (16.104)	-39.108* (23.598)
18 months	0.030 (0.025)	0.054* (0.032)			
24 months	0.035 (0.022)	0.064** (0.030)			
<i>N</i>	45,562	24,961	<i>N</i>	45,562	24,961

*Notes:* UI entry-date-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the local ATE coefficients but control for all covariates using our main specification. Results in the first column are based on individuals who are 20 to 52 years old. The second column refers to individuals who are 35 to 52 years old. Both samples are restricted to individuals entitled to more than 180 days of UI benefits who entered their UI spell between 1 July 2011 and 31 July 2013 (medium sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

The overall local ATEs are positive regarding our *within outcome measures* of the employment probability. In this case, restricting the sample to individuals who are 35 to 52 years old translates into ATEs that almost double in size and turn slightly significant. We find insignificant effects on the total employment probability, as it covers both the positive effects on the probability of finding a job, and the counteracting effects on the self-employment probability. More detailed RDD results are shown in Appendix Tables II.11 and II.12.

In line with the DiD results, the reform's local effect on the actual UI duration is negative but insignificant. We find a significantly negative effect with respect to the general unemployment (UE) duration. Even though the levels are different, we observe a very similar pattern of the UI and UE duration elasticities (see Appendix Table II.14) calculated from the RDD estimates compared with those obtained from the DiD (see Table 2.4). Again, differences in levels of the elasticities across both estimation methods may be explained by the fact that we rather measure local effects in the RDD setting.

### RDD Visualized Results

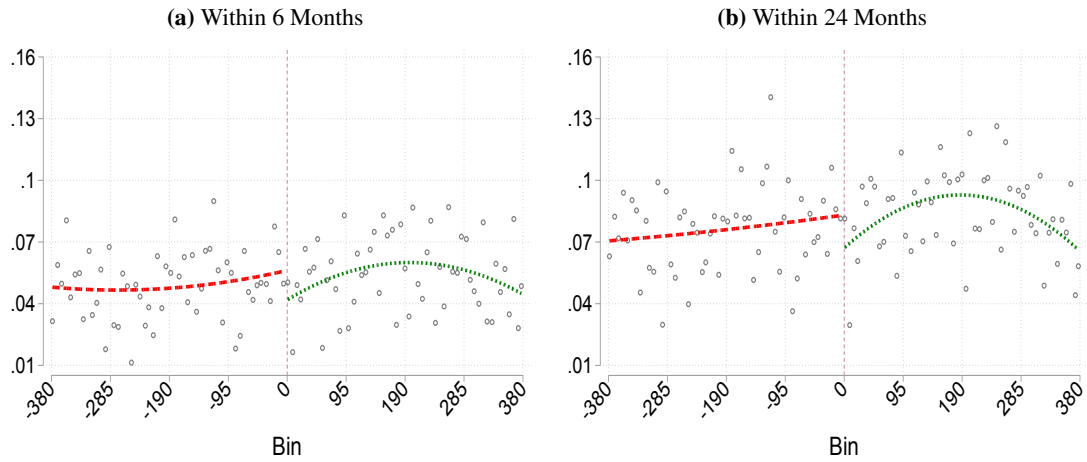
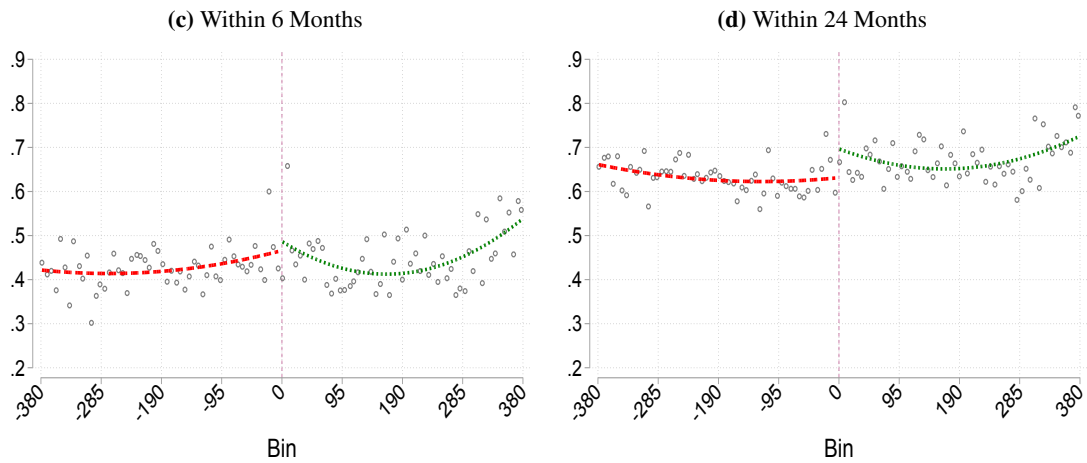
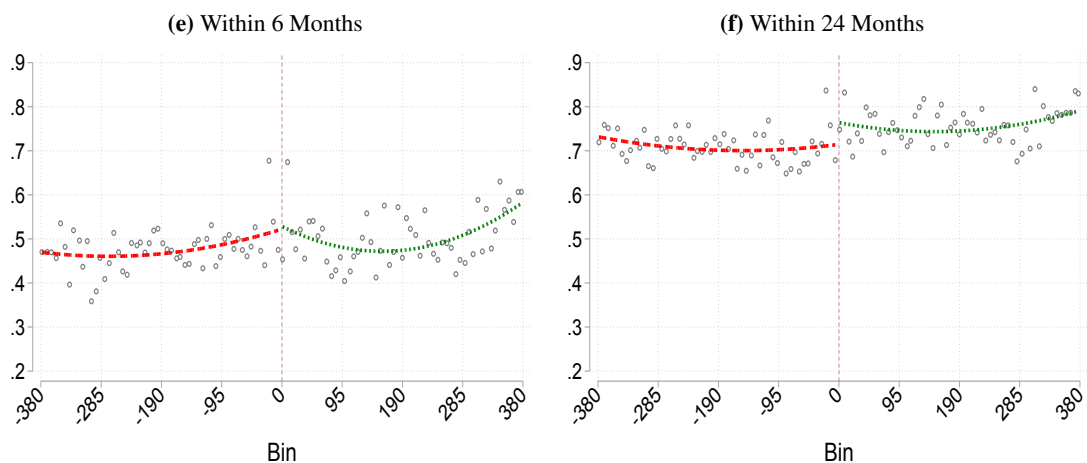
Our main RDD results are visualized in Figure 2.8. It plots the jump in the average unemployment exit probabilities after the cutoff date. We only consider the within-probabilities for the short and long run. The remaining results are very similar, though. The quadratic set-up of our RDD fits the data pretty well. The negative reform effects on self-employment as well as the positive effects on (total) employment are visible. The jumps intensify in the long run – regardless of the outcome variable. More detailed versions of these results and a comparison of findings for 20-52 and 35-52 years old individuals can be inferred from Appendix Figures II.23 to II.25. There are only few differences across the result specifications, which indicates their robustness.

### RDD Robustness Checks

It is worth to mention a few findings concerning robustness. Our RDD results turned out to be insignificant when using the large sample, but those results point towards the same direction as our medium sample based main results.<sup>36</sup> When using a smaller sample (see Table II.13) with a bandwidth of only six months, we obtain almost the same results. But they turn out to be even more significant than those for the medium sample.

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<sup>36</sup>The RDD regression results for the large sample are provided upon request.

**Figure 2-8: RDD Short- and Long-Run Reform Effects****(1) Effect on Self-Employment Probabilities****(2) Effect on Employment Probabilities****(3) Effect on Total Employment Probabilities**

*Notes:* These figures illustrate the RDD results for our quadratic prediction plots of exit state probabilities. The sample is restricted to individuals who are 35 to 52 years old, entitled to more than 180 days of UI benefits. We only include individuals whose UI benefit spell starts between 1 July 2011 and 31 July 2013 (medium sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

### RDD Placebo Tests

We show placebo tests of our RDD in Table 2.8 to provide further support on the reliability of our identification strategy. Again, column (1) shows our main specification for the sample of individuals who are 35 to 52 years old. Column (2) tests the effect of a placebo reform that is set to artificially take place in July 2011. The third column refers to our estimates when we only include individuals unaffected by the reform because they are entitled to no more than six months of UI benefits. By generating insignificant estimates both placebo tests support the reliability of the RDD identification strategy.

**Table 2.8:** RDD Placebo Tests

Outcomes	(1)	PLACEBO TESTS	
		(2)	(3)
<i>Self-Employment</i>			
6 months	-0.024* (0.015)	-0.006 (0.009)	-0.005 (0.009)
12 months	-0.028* (0.016)	-0.004 (0.010)	0.009 (0.015)
18 months	-0.029* (0.017)	-0.007 (0.010)	0.020 (0.017)
24 months	-0.033** (0.016)	-0.003 (0.011)	0.022 (0.017)
<i>Employment</i>			
6 months	0.064 (0.040)	0.038 (0.046)	0.040 (0.050)
12 months	0.066* (0.037)	0.026 (0.024)	0.027 (0.048)
18 months	0.054* (0.032)	0.005 (0.024)	-0.005 (0.047)
24 months	0.064** (0.030)	-0.007 (0.023)	0.025 (0.043)
<i>Total Employment</i>			
6 months	0.040 (0.036)	0.036 (0.023)	0.036 (0.050)
12 months	0.038 (0.032)	0.023 (0.021)	0.035 (0.048)
18 months	0.025 (0.028)	-0.001 (0.021)	0.015 (0.045)
24 months	0.030 (0.023)	-0.010 (0.020)	0.047 (0.041)
<i>N</i>	24,961	50,176	6,994

*Notes:* UI entry-date-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the local ATE coefficients but control for all covariates using our main specification. For comparison reasons, column (1) presents the results for our main setting using the medium sample (see Table 2.7). In column (2), we set the reform to take place at a fictive date on 1 July 2011. Column (3) reports the placebo test results if we only include individuals unaffected by the reform because they are entitled to no more than 6 months of UI benefits. All samples are restricted to individuals who are 35 to 52 years old.

*Source:* Authors' calculations based on MCVL 2005-2017 data.



## 2.7 Discussion and Interpretation of Results - Potential Mechanisms

### 2.7.1 Summary and Discussion of Main Results

Our results show that a reduction of **UI** benefits affects the transition out of unemployment. On the one hand, both **DiD** and **RDD** show that the probability to become re-employed increases in the short run. Thereby, a behavioral pattern in line with Search Theory can be observed: in expectation of the drop in **UI** benefits, unemployed individuals increase their search efforts and are thus more likely to leave **UI** towards re-employment before the drop in **UI** benefits takes effect after six months. This result seems to confirm findings of [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#).

On the other hand, the effect of the cut in **UI** benefits appears to be different for the transition into self-employment. Our results suggest that the probability to transfer from unemployment to self-employment is less affected (**DiD** results) or even slightly declines (**RDD** results). This indicates that the reduction in **UI** benefit levels does not push unemployed individuals to become self-employed, but rather induces search for employment on the extensive margin. However, it also reduces actual **UI** duration of those transitioning into self-employment, and thus may affect new startups.

Moreover, the **UI** duration elasticity for those unemployed individuals that become self-employed is smaller (around 0.25 in **DiD**, up to 0.5 in **RDD**) than for those becoming employed (around 0.4 in **DiD**, up till 0.9 in **RDD**). Hence, **UI** benefit levels have less effect on the flow from unemployment to self-employment than for the channel from unemployment into employment. Regarding our **UI** duration elasticity, one should note that we can only compare our estimate with respect to employment to other estimates in the literature. Given the fact that we analyze a cut in **UI** benefit levels, the estimate of around 0.4 appears to be in line with other estimates ([Doris et al. 2018](#)). However, our estimate is much smaller than the one reported by [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#). This may be related to the fact that we are able to evaluate the long-run reform effect, and that we take into account total employment, construct a more representative sample, and do not only limit on full-time employment after 64 weeks. In conclusion, **UI** benefit levels affect the actual **UI** duration for those transferring into re-employment more than for those who decide to start a business out of unemployment.

Our main findings are in line with the predictions from Standard Search Theory which suggests that a decrease in benefit levels would lead to higher search intensity (compare also to [Marinescu & Skandalis 2019](#)). Thus, the reservation wage for employment would decrease. In general equilibrium, vacancies may increase and therefore labor market tightness rises, which means that we would expect a higher job-finding rate. In fact, this is what we find in our empirical results for the short term (see [Figure 2.7](#)). However, as employment may rise, self-employment may be unaffected or becomes less likely in relative terms. This reasoning is in line with our **DiD** results which estimate the average treatment effect to be close to zero ([Section 2.6.1](#)).



Turning to our RDD results in Section 2.6.2, we find a significantly negative local treatment effect on the self-employment probability. This finding is in line with the Entrepreneurial Choice Model which predicts that shortened UI duration, caused by the decrease in benefits, leads to less human capital deterioration and relatively better employment prospects compared with an unchanged UI benefit level. As there is less time for learning about proper business opportunities, it is easier to find a job than starting a business.

In summary, we find a significantly negative local average treatment effect (RDD) on the probability to become self-employed but a zero average treatment effect (DiD). The DiD approach compares treated individuals (with more than six months of UI entitlement) before and after the reform with individuals from the control group (with at most six months of UI entitlement) before and after the reform. The RDD approach only considers the local difference of individuals in the treatment group (with more than six months of UI entitlement) around the cutoff date. This suggests that the additional difference used in the DiD absorbs most of the negative effect that we estimate in our RDD.

### 2.7.2 Welfare Analysis - Potential Mechanisms

From a policy perspective, it is interesting to understand the welfare implications of the estimated causal results on the transition into self-employment. In the following, we provide evidence whether the quality of self-employment has changed in response to the reform, thus revealing the potential direction of the welfare effect.

Figure II-14 shows that the average share of self-employment among different age groups before and after the reform has changed. Older individuals are less often self-employed than before, whereas the opposite can be observed with respect to the younger generation. However, this is only descriptive evidence because it could be caused by other reforms in 2013 which particularly target young unemployed individuals to become self-employed (see Appendix II.4.6 for more details about reforms).

We also find some descriptive evidence for the change in self-employment quality once we analyze changes in the sector classification of the self-employed. Figure II-13 shows a remarkable decreasing trend in the construction sector during the years of the recession until 2013 when the economy becomes stable again. The distributions of agricultural, industrial, finance/real estate, and information/communication sectors virtually stay constant over time. Most interesting is the rise of the service sector and the sector of professional activities after the crisis but also during the years after the reform. Especially, a growing service sector which includes transport, tourism and retail activities could indicate an increase in *necessity entrepreneurship*.

The mean comparison in Appendix Table II.15 shows that among treated individuals there is indeed a significant difference between the sector in which individuals worked before they became unemployed and the new sector in which they run a their business.

**Table 2.9:** Welfare Analysis: Sectors of New Self-Employment

	Agr.	Ind.	Constr.	Serv.
<b>DiD</b>	0.004 (0.023)	-0.025 (0.032)	-0.058 (0.078)	0.103 (0.090)
<i>N</i>	752	752	752	752
<b>RDD</b>	-0.010 (0.012)	-0.037 (0.044)	-0.018 (0.062)	0.027 (0.091)
<i>N</i>	1,224	1,224	1,224	1,224

*Notes:* Region-clustered (**DiD**) and entry-date-clustered (**RDD**) standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We apply **DiD** and **RDD** estimation methods to derive results using different self-employment sector indicators (agricultural, industrial, construction, and service sector) as dependent variables. These variables refer to the sector of activity in the self-employment spell right after unemployment. We use the specification in which we control for all covariates, but we only report the **ATE** (**DiD**) and local **ATE** (**RDD**) coefficients respectively. We use the large sample for the **DiD** and the medium sample for the **RDD**, restricted to individuals who are 35 to 52 years old and who exit from unemployment into self-employment within 24 months.

*Source:* Authors' own calculations based on **MCVL** 2005-2017 data.

We observe that the share of treated individuals who started to work as self-employed in the service sector significantly increases at the expense of the industry and construction sector. Again, this could be interpreted as evidence that the reform may have fostered *necessity*<sup>37</sup> entrepreneurship rather than *opportunity* entrepreneurship.

So far we only considered correlations. In the following, we try to disentangle the potential causal relationship between the cut in **UI** benefits and self-employment quality estimating the same **DiD** and **RDD** specifications as described in **Section 2.5** but using outcome variables for (self-)employment quality. We measure (self-)employment quality using industry classification, the subsequent (self-)employment duration in days, as well as the social security contribution basis as best available proxy for self-employment income<sup>38</sup>. By restricting our samples to individuals who transition into (self-)employment when they exit their unemployment spell, they are much smaller than the samples used in our main specifications above.

**Table 2.9** above shows the regression results using different self-employment sector indicators as outcome variables. Clearly there is no significant impact of the reform on self-employment sector choice, neither in the **DiD** nor in the **RDD** setting. **Table 2.10** shows that we obtain similar results if we use earnings as outcome variable, approximated by the social security contribution basis. The effect on self-employment income is mixed: it seems to be negative in the **DiD** but positive using the **RDD**. But both effects are insignificant. This mixed result may be related to the fact that for self-employed individuals the social security contribution basis is not ideal to approximate actual earnings.

<sup>37</sup>Fairlie & Fossen (2018) show that this type of self-employment follows a strongly counter-cyclical pattern and is moving with the national unemployment rate.

<sup>38</sup>Note that, in case of employment, the social security contribution basis corresponds to the average spell-specific monthly earnings.

**Table 2.10:** Welfare Analysis: Exit Spell Earnings and Duration Regressions

	EXIT SPELL EARNINGS				EXIT SPELL DURATION			
	SE 24	SE 12	E 24	E 12	SE 24	SE 12	E 24	E 12
<b>DiD</b>	-13.421 (13.536)	-15.600 (17.392)	-26.539 (23.698)	-36.400 (24.488)	-19.524 (49.115)	11.202 (76.614)	2.902 (11.549)	-3.224 (13.370)
<i>N</i>	1,302	1,068	13,081	11,045	1,302	1,068	13,165	11,109
	SE 12	SE 6	E 12	E 6	SE 12	SE 6	E 12	E 6
<b>RDD</b>	16.557 (74.004)	22.495 (90.282)	-1.477 (66.631)	3.355 (72.350)	50.041 (86.758)	4.007 (88.435)	-17.019 (27.356)	-30.381 (29.320)
<i>N</i>	1,648	1,314	13,639	10,870	1,648	1,314	13,723	10,941

*Notes:* Region-clustered (**DiD**) and **UI** entry-date-clustered (**RDD**) standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We conduct **DiD** and **RDD** estimations using two different outcome variables: 1) spell-specific monthly social security contribution basis to approximate (self-)employment earnings and 2) spell duration (in days), right-censored to 1,080 days (3 years). Both dependent variables refer to the spell right after unemployment (either employment or self-employment). We use the specification in which we control for all covariates, but we only report the **ATE** (**DiD**) and local **ATE** (**RDD**) coefficients respectively. We use the large sample for the **DiD** and the medium sample for the **RDD**, restricted to individuals who are 35 to 52 years old. We only include those that exit from unemployment into (self-)employment. For the **DiD**, we consider transitions into (self-)employment within 12 and 24 months. For the **RDD**, transitions within 6 and 12 months are considered.

*Source:* Authors' own calculations based on **MCVL** 2005-2017 data.

Regarding employment, the negative effect is at least persistent across both models, but again insignificant.<sup>39</sup> The right-hand side of **Table 2.10** refers to our regression results using spell duration of the exit state as outcome variable. Again, the results from both models are mixed and highly insignificant considering the huge standard errors.

All in all, we find descriptive evidence but no causal reform effect on self-employment quality. As all our quasi-experimental estimation results are insignificant, the reform does not seem to affect the quality of self-employment which means that the composition of self-employment does not change due to the reform. Consequently, potential welfare implications seem to be limited to the increase in the job-finding rate but with a much smaller degree than proposed by **Rebollo-Sanz & Rodríguez-Planas (2020)**. This suggests that ignoring self-employment and only focusing on results for employment instead of total employment may lead to biased conclusions.

<sup>39</sup>Note that these apparently negative effects might be related to changes in the sector, rather than (self-)employment experience, as suggested by **Kaiser & Malchow-Møller (2011)**.

## 2.8 Conclusion

This second chapter of my dissertation addresses how a reduction in **UI** benefit levels affects total employment, i.e. the probability to become (re-)employed. Thereby, we distinguish between self-employment and dependent employment. We investigate how this reduction affects actual unemployment duration before unemployed individuals become self-employed, as compared to those who become re-employed. Furthermore, we also present approaches to disentangle the causal effect on self-employment by analyzing its consequences on different outcomes of self-employment quality, in order to investigate potential welfare implications. Finally, we rationalize our findings in relation to the existing literature in labor and public economics.

While the existing literature has addressed how **UI** policies affect unemployment duration and re-employment wages when self-employment as post-unemployment exit state is excluded, we are among the first to include it. Since active labor market policies, which incentivize mainly long-term unemployed individuals to become self-employed, are commonly used policy measures to fight unemployment, understanding the effects of the design of **UI** policies on self-employment seems to be highly relevant.

To surpass data limitation on the labor market employment histories of founders, we prepare Spanish administrative social insurance and labor income data to analyze all relevant labor market flows over the business cycle (2005-2017). This allows us to provide a descriptive analysis of self-employment in Spain, both with respect to stock and flow dimension. We show that flows from unemployment to self-employment are very relevant in the case of Spain: 30% of all new businesses are created by previously unemployed founders. During the crisis, this share increases up to 50% for some years.

We exploit a Spanish labor market reform in 2012 which affected the **UI** benefit schedule by reducing the existing replacement rate of an individual's previous net income by approximately 16.67%. Using both **RDD** and **DiD** methods, we identify the causal effects of the reduction in **UI** benefits on the transition probabilities and on actual unemployment duration by exploiting reform-based exogenous variation in **UI** benefit level schedules within the Spanish **UI** system.

Our **DiD** results show that on average a reduction in **UI** benefits does not significantly affect the self-employment probability, neither in the short run (within six months of **UI** entry) nor in the medium and long run (within 12-24 months of **UI** entry). If we focus on the local average treatment effect identified by our **RDD**, we find a significantly negative effect on the self-employment probability expanding in size throughout **UI** spell duration.

On the contrary, our estimated average treatment effect on the probability to become (re-)employed seems to be positive in the short run. It converges towards zero in the medium run and turns significantly negative in the long run when we consider the whole period. The local average treatment effect on employment is estimated to be significantly positive throughout the UI spell. Our estimates' magnitude is much smaller than the dimension of the estimates provided by [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#). They estimate a (local) average treatment effect on the job-finding rate of (26%) 41% in their (RDD) DiD while our estimates point towards a short-run (local) average treatment effect of (6.4%) 2.7%. This shows that the exclusion of data on self-employment matters and may lead to substantial overestimation bias.

In line with [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#), our DiD results clearly show a behavioral response of the treated individuals: unemployed individuals increase their search intensity to find employment within the first six months of unemployment, anticipating the drop in UI benefits after six months. This explains the increase in the short-term employment rate and its decline after the first six months. We find a much smaller reaction to changes in UI benefits when it comes to self-employment. Consequently, the probability to become self-employed declines in relative terms. Our DiD results are in line with Standard Search Theory for both employment as well as self-employment. Moreover, the local treatment effect (as measured by the RDD) speaks in favor of a significantly negative effect on the self-employment probability which is supported by the Entrepreneurial Choice Model.

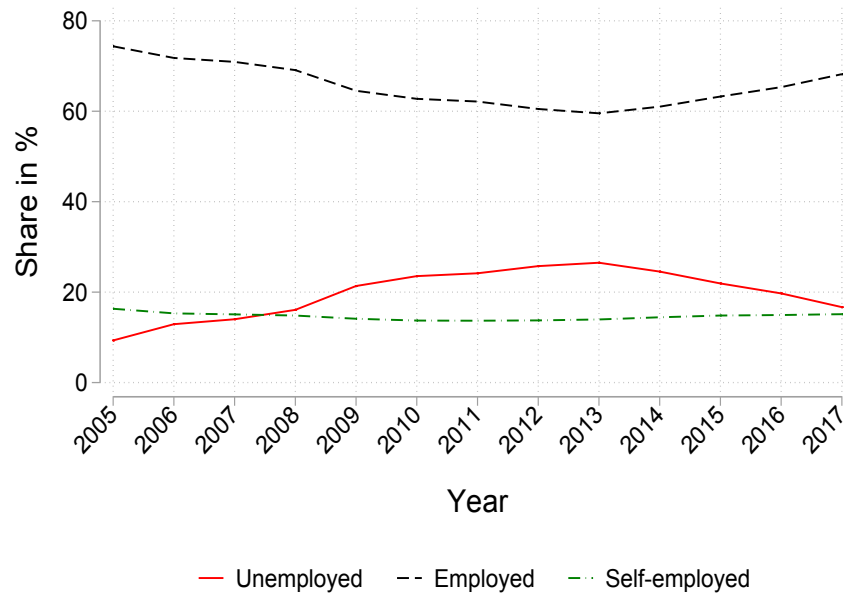
In line with the findings of [Doris et al. \(2018\)](#) and [Rebollo-Sanz & Rodríguez-Planas \(2020\)](#), we confirm that the UI benefit level duration elasticity is larger with respect to UI benefit level cuts (rather than increases). We also find that the UI duration elasticity is larger for those exiting into employment compared to those transitioning into self-employment. Taking stocks including the results derived in [Chapter 1](#) of my dissertation (which corresponds to the discussion paper of [Camarero Garcia & Murmann 2020](#)), time seems to be more important than money when it comes to the effect on self-employment. [Camarero Garcia & Murmann \(2020\)](#) show that the extension of potential UI benefit duration prolongs actual unemployment duration of those becoming self-employed. In this case, the UI duration elasticity for those transitioning to self-employment is higher than common estimates for those becoming re-employed suggest.

While we find some descriptive evidence for changes in self-employment quality due to the reform that speaks in favor of an increase in *necessity-driven* entrepreneurship, we cannot confirm a causal relationship. Future research may help to investigate the welfare implications of reducing UI benefits on self-employment in more detail. Moreover, theoretical models would help to rationalize our findings, thereby taking into account both the effect of PBD on start-up success ([Camarero Garcia & Murmann 2020](#)), as well as the effect of UI benefit levels on the probability to exit into self-employment, as discussed in this second chapter of my doctoral thesis.

## II.1 Appendix: Supplementary Figures

### II.1.1 Descriptive Analysis Figures

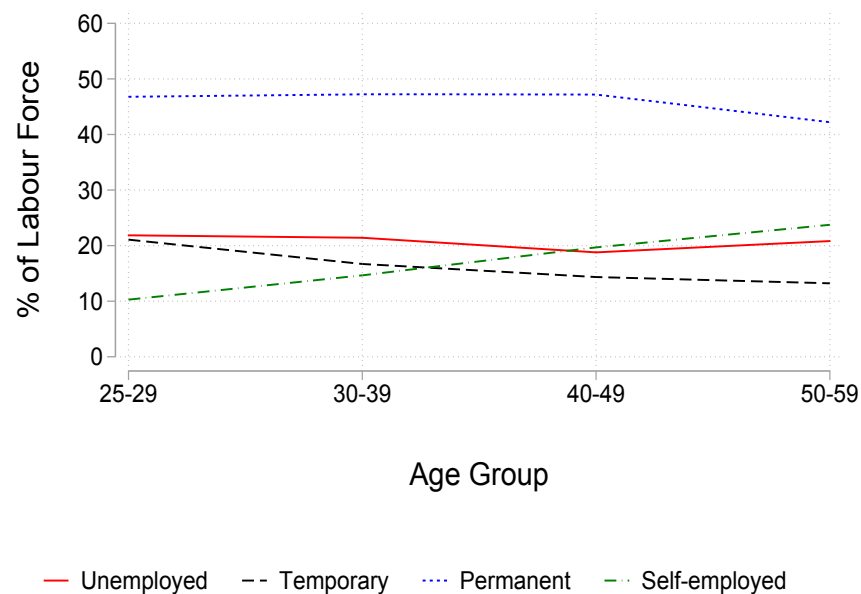
**Figure II.1:** Composition of the Labor Force in Spain



*Notes:* This figure illustrates the composition of the Spanish labor force between 2005 and 2017. It shows the percentage of individuals of working age (in this case, over 18 years old) distinguishing Unemployment, Dependent Employment and Self-Employment.

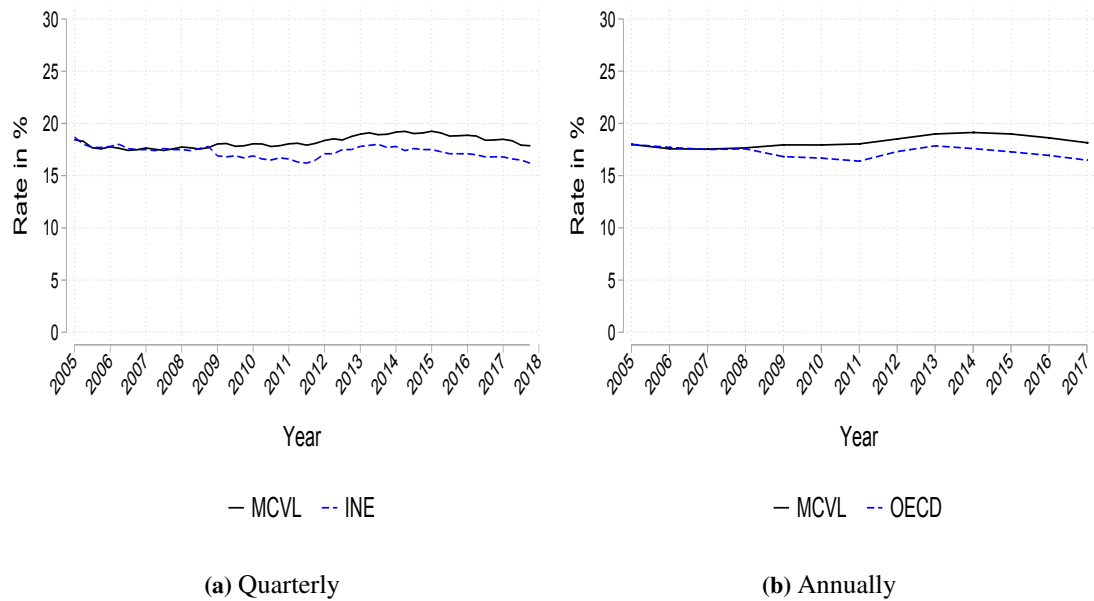
*Source:* Authors' calculations based on the [MCVL](#) 2005-2017 data.

**Figure II.2:** Distribution of Workers across Employment States and Age Groups

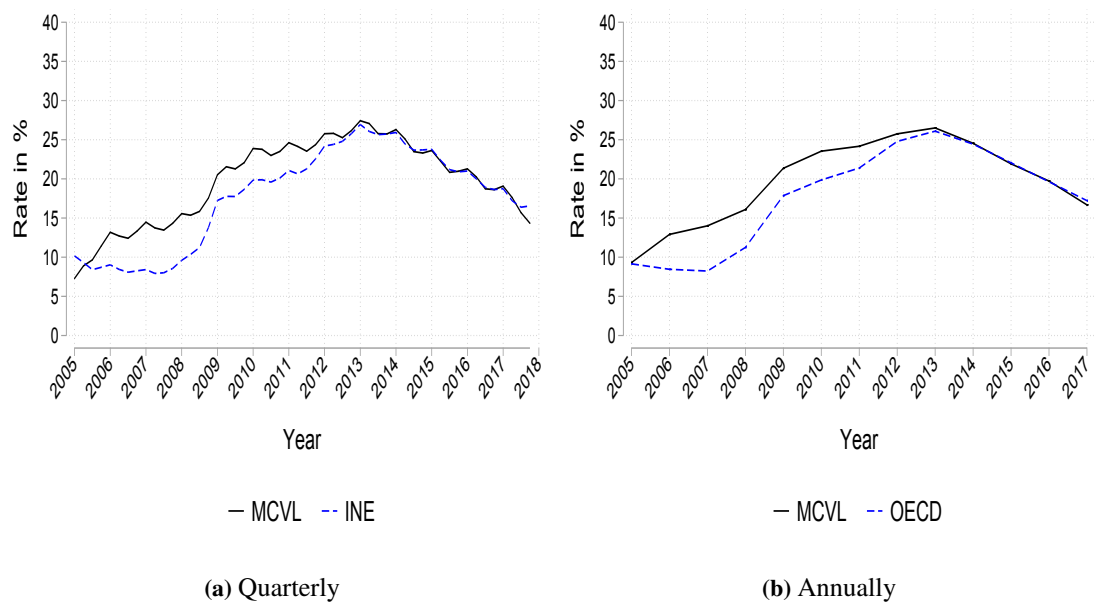


*Notes:* This figure illustrates the distribution of workers across the different employment states: Unemployment, Temporary Employment, Permanent Employment and Self-Employment, with respect to their age group, as a percentage of the Spanish labor force.

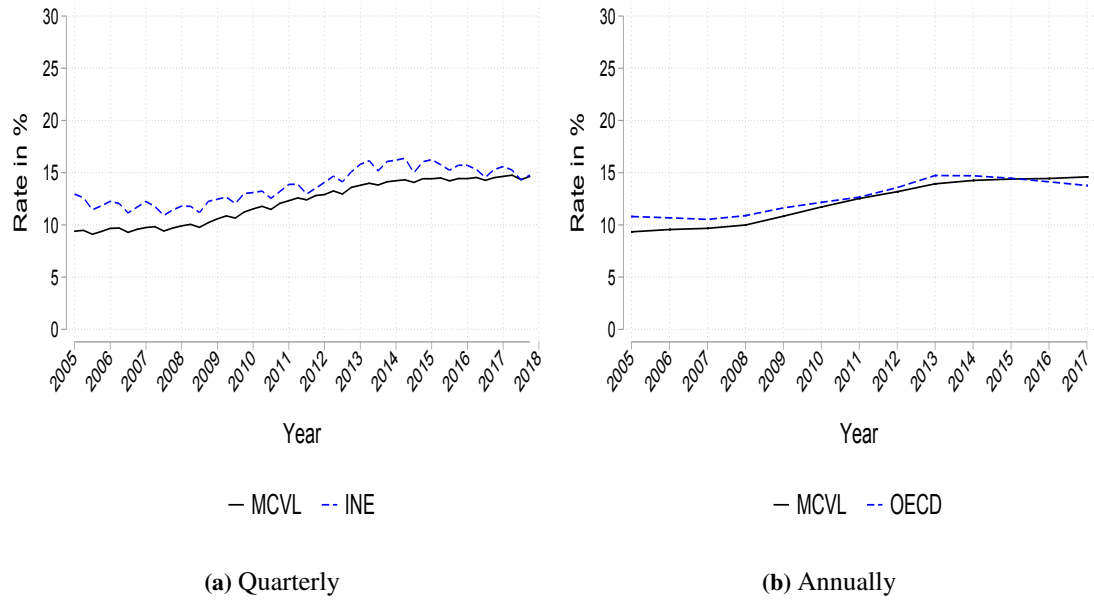
*Source:* Authors' calculations based on the [MCVL](#) 2005-2017 data.

**Figure II.3: Self-Employment Rate**

*Notes:* The left-hand figure illustrates the evolution of the self-employment rates in Spain from 2005 to 2017 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.  
*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data and official statistics provided by [INE](#) (2018) and [OECD](#) (2018).

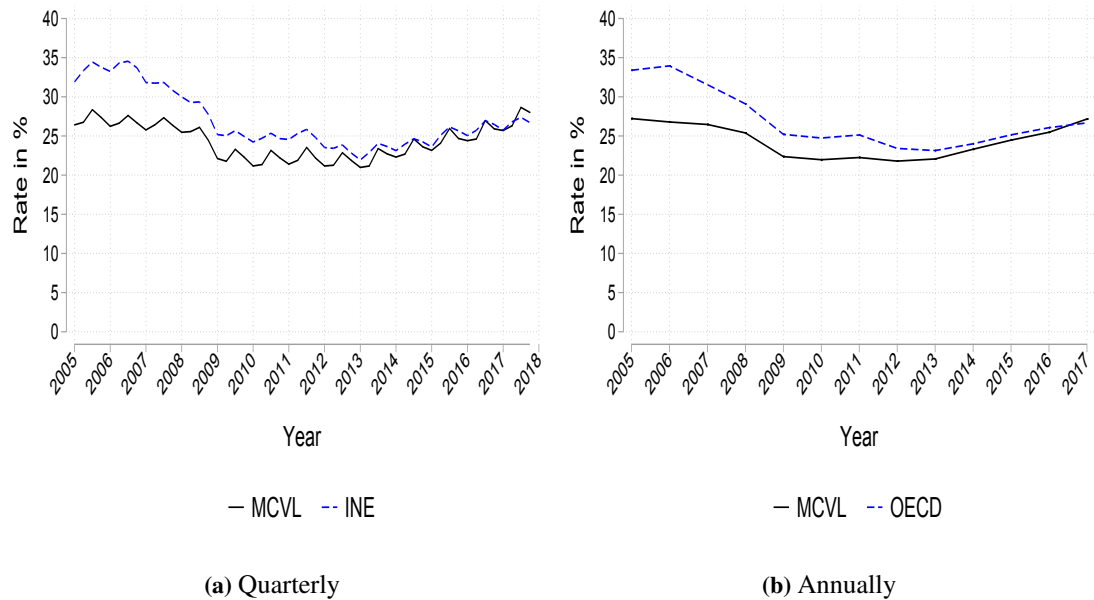
**Figure II.4: Unemployment Rate**

*Notes:* The left-hand figure illustrates the evolution of the unemployment rates in Spain from 2005 to 2017 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis. Note that our definition of unemployment includes individuals who receive either contributory (**UI**) or non-contributory (**UA**) unemployment benefits, as well as individuals who do not receive any benefits at all, and those who are tagged as receiving cease-of-activity benefits.  
*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data and official statistics provided by [INE](#) (2018) and [OECD](#) (2018).

**Figure II-5: Part-Time Employment Rate**

*Notes:* The left-hand figure illustrates the evolution of the part-time employment rates in Spain from 2005 to 2017 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.

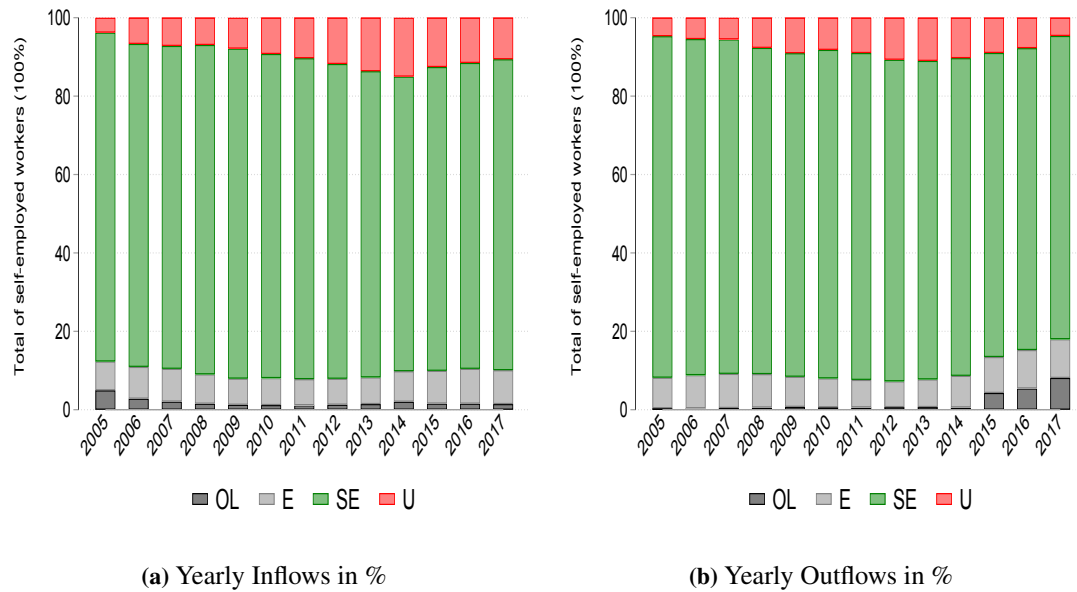
*Source:* Authors' calculations based on MCVL 2005-2017 data and official statistics provided by INE (2018) and OECD (2018).

**Figure II-6: Temporary Employment Rate**

*Notes:* The left-hand figure illustrates the evolution of the temporary employment rates in Spain from 2005 to 2017 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.

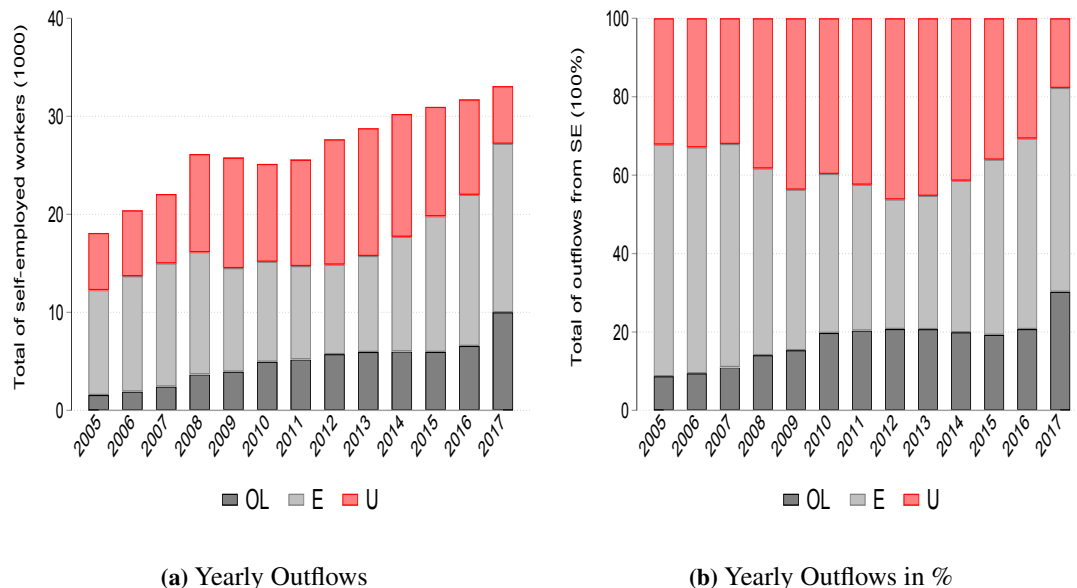
*Source:* Authors' calculations based on MCVL 2005-2017 data and official statistics provided by INE (2018) and OECD (2018).



**Figure II-7: Composition: Inflows into/Outflows from Self-Employment incl. Stocks**

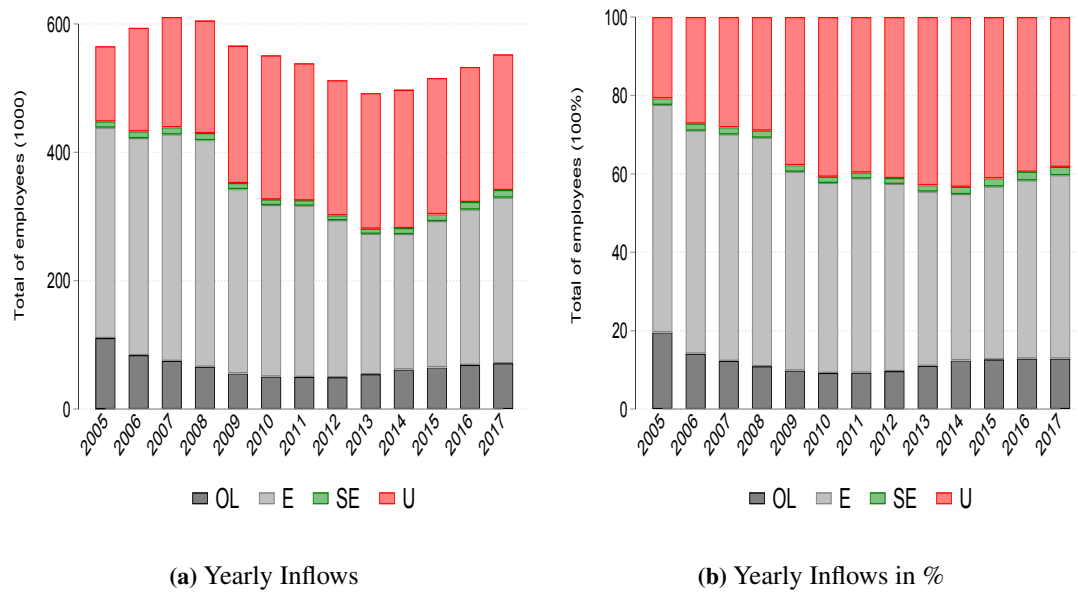
*Notes:* These figures illustrate the yearly composition of self-employment in Spain providing the share of each component in percentage of the total stock. We distinguish transitions to self-employment (inflows), on the left-hand side, and transitions from self-employment (outflows), on the right-hand side, with respect to the following labor market states: out of the labor force (OL), dependent employment (E), unemployment (U), and the corresponding stock of those who remain in self-employment (SE). The sample is restricted to individuals of working age (in this case, over 18 years old).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-8: Composition of Outflows from Self-Employment excl. Stocks**

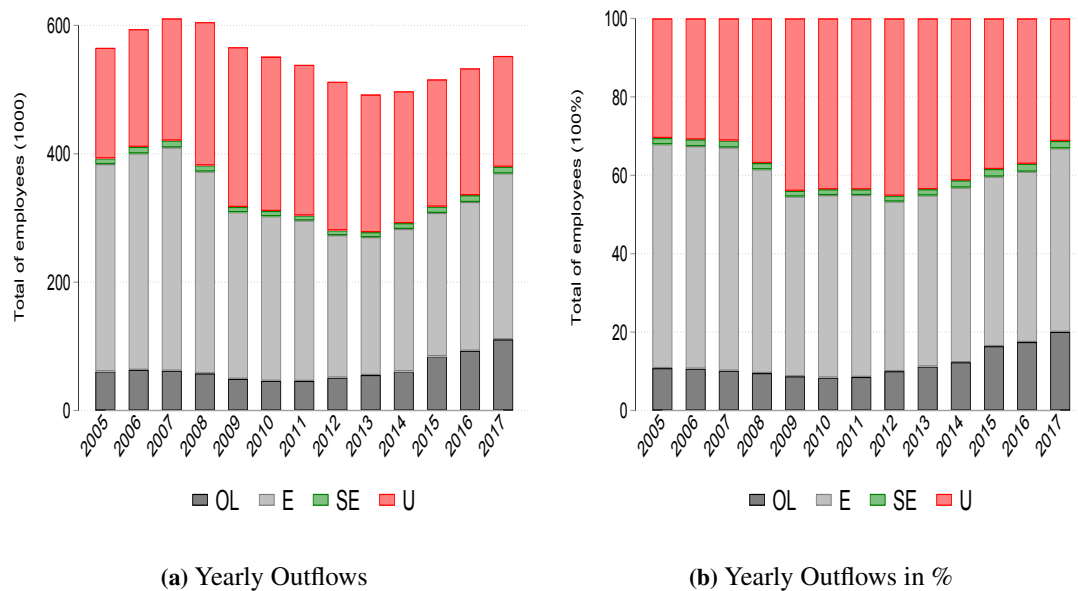
*Notes:* These figures illustrate the yearly outflows from self-employment in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, over 18 years old). We distinguish outflows of individuals from self-employment (SE) to the following labor market states: out of the labor force (OL), dependent employment (E), and unemployment (U). This is the other side of the coin: the inflows are shown in the main text in Figure 2-1.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-9: Composition of Inflows into Employment**

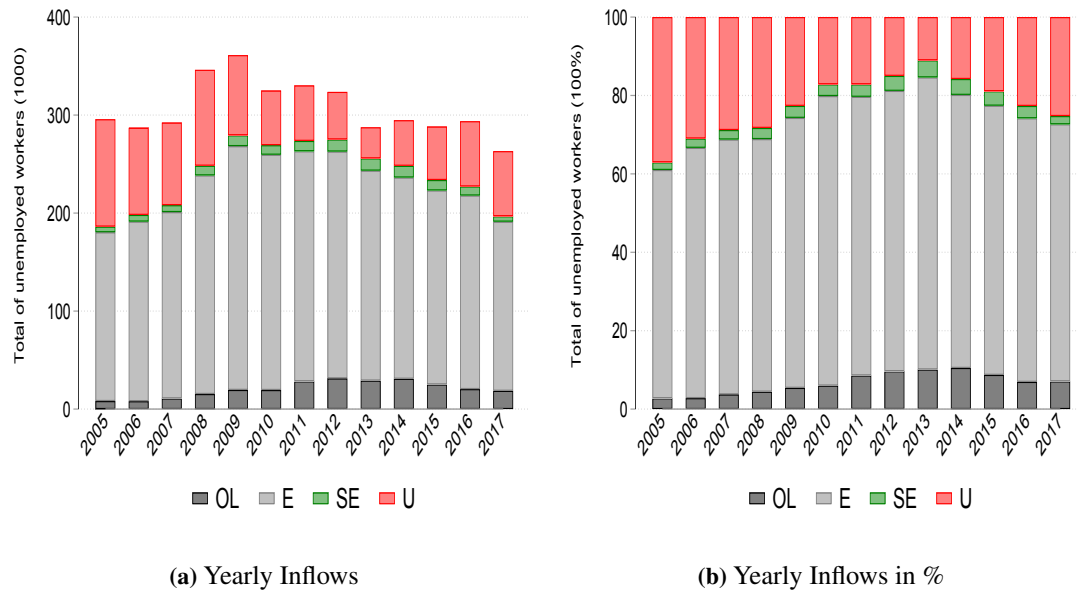
*Notes:* These figures illustrate the yearly composition of transitions to employment (inflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, over 18 years old). We consider inflows of individuals into employment (E) from the following labor market states: out of the labor force (OL), self-employment (SE), and unemployment (U), along with the corresponding stock of those who remain in dependent employment (E).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-10: Composition of Outflows from Employment**

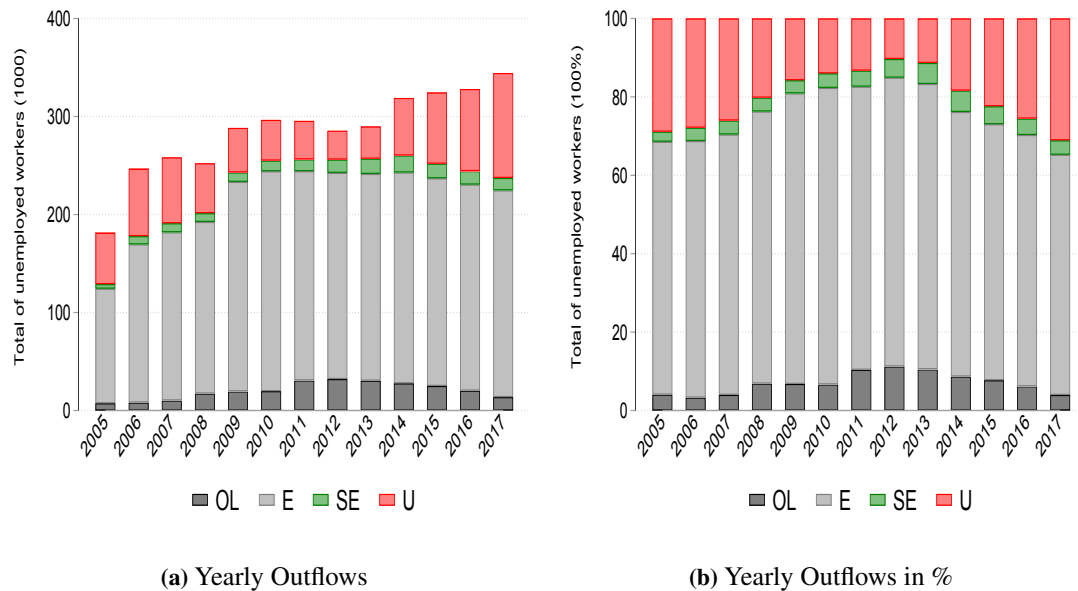
*Notes:* These figures illustrate the yearly composition of transitions from employment (outflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, over 18 years old). We consider outflows of individuals from employment (E) into the following labor market states: out of the labor force (OL), self-employment (SE), and unemployment (U), along with the corresponding stock of those who remain in dependent employment (E).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-11: Composition of Inflows into Unemployment**

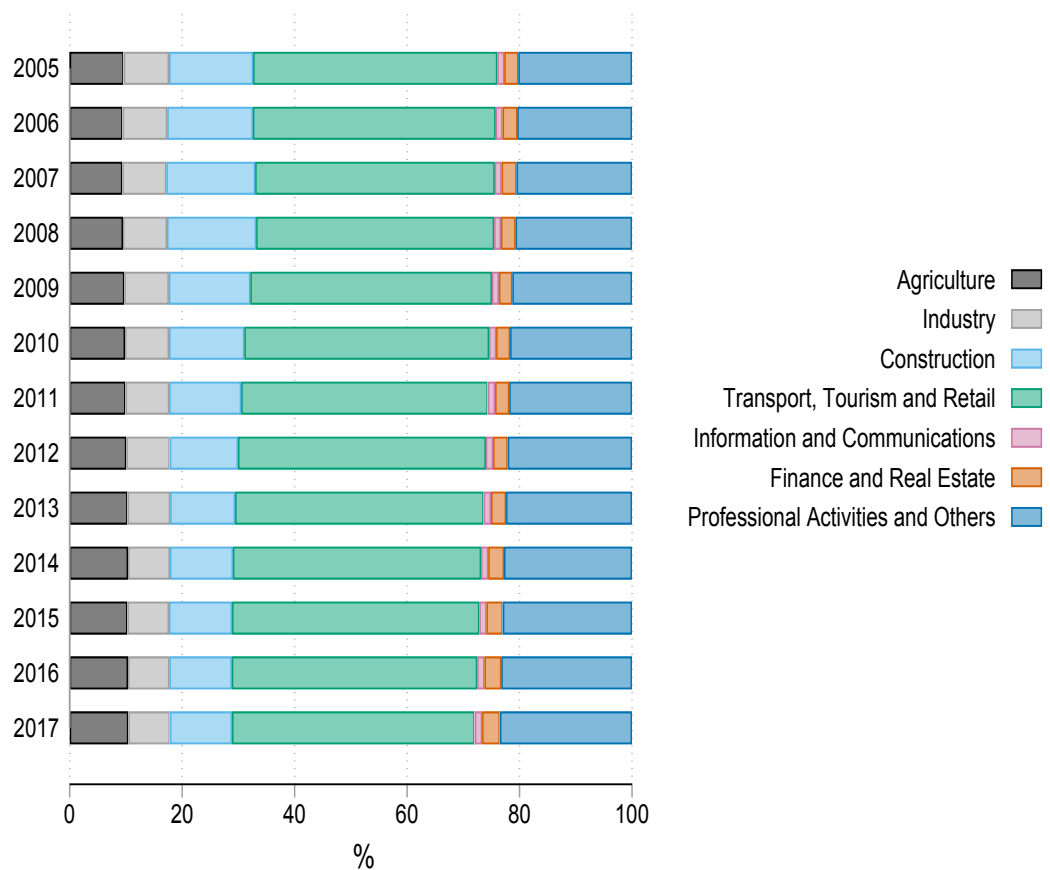
*Notes:* These figures illustrate the yearly composition of transitions to unemployment (inflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, over 18 years old). We consider inflows of individuals into unemployment (U) from the following labor market states: out of the labor force (OL), dependent employment (E), and self-employment (SE), along with the corresponding stock of those who remain in unemployment (U).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-12: Composition of Outflows from Unemployment**

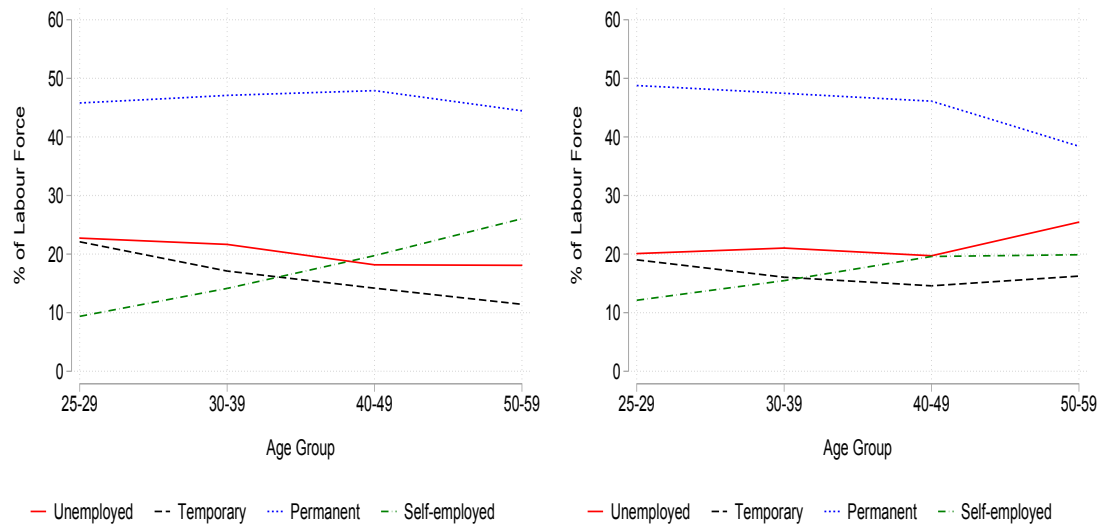
*Notes:* These figures illustrate the yearly composition of transitions from unemployment (outflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, over 18 years old). We consider outflows of individuals from unemployment (U) into the following labor market states: out of the labor force (OL), dependent employment (E), and self-employment (SE), along with the corresponding stock of those who remain in unemployment (U).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II.13: Sector Distribution of the Self-Employed**

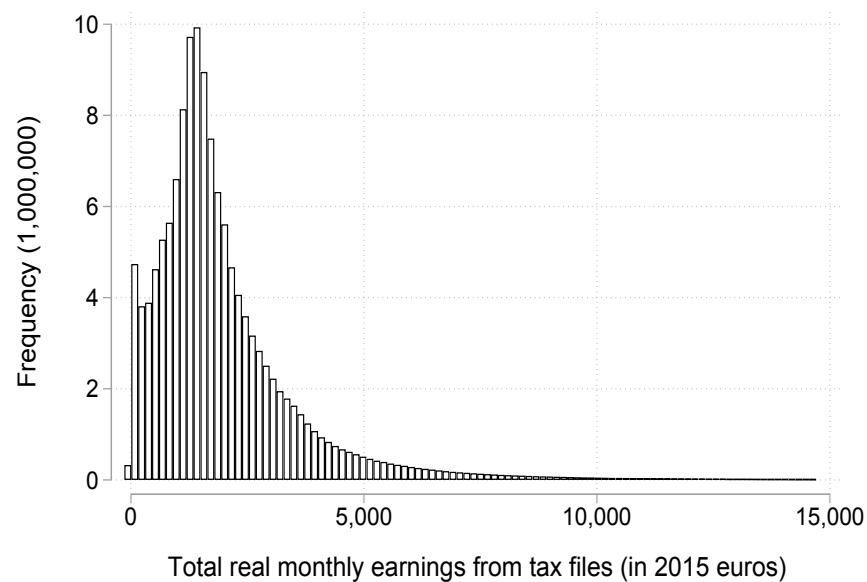
*Notes:* This figure illustrates the composition of self-employment in Spain, with respect to the sector variable in each year. The sample is restricted to individuals who are 35 to 52 years old.

*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data and on the classification in [García & Román \(2019\)](#).

**Figure II·14:** Distribution of Workers across Employment States and Age Groups**(a) Before the Labor Market Reform (2005-2012)      (b) After the Labor Market Reform (2013-2017)**

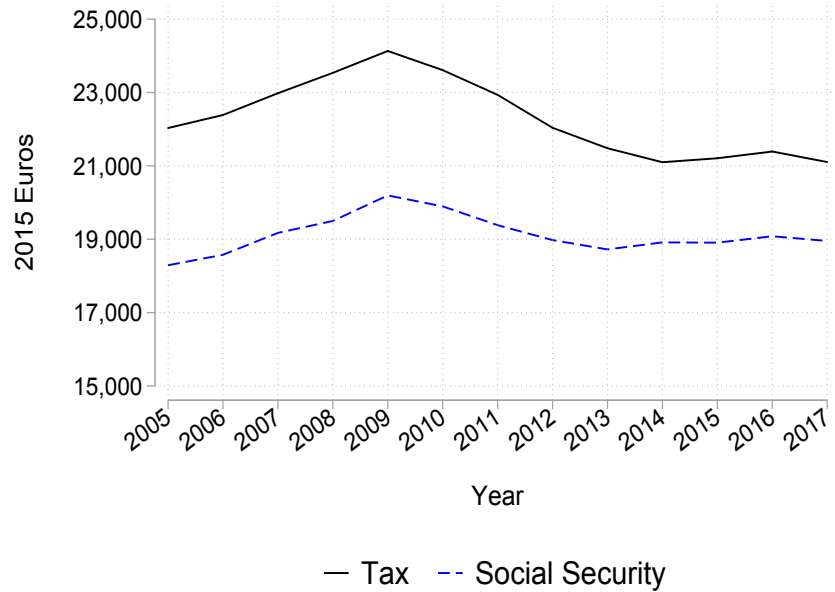
*Notes:* These figures illustrate the distribution of workers across the different employment states: unemployment, temporary employment, permanent employment and self-employment, with respect to their age group, as a percentage of the Spanish labor force. The share of self-employed among older individuals (50 and older) appears to decline in favor of unemployment and part-time employment, whereas for the youth (below 30) self-employment becomes more relevant.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II·15:** Distribution of Monthly Earnings (Tax Data)

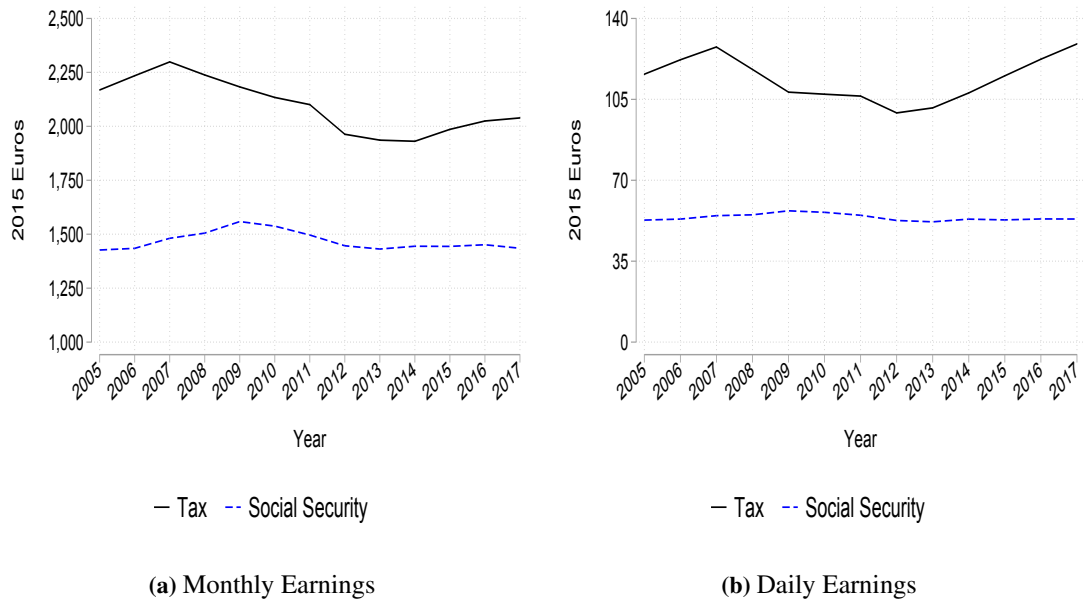
*Notes:* This figure illustrates the distribution of real monthly earnings in Spain, according to the tax files. The sample is restricted to individuals who are older than 18.

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-16: Evolution of Yearly Earnings**

*Notes:* This figure illustrates the evolution of mean annual earnings in Spain for Employment, according to the Social Security records and the tax files. The sample is restricted to individuals who are older than 18.

*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data.

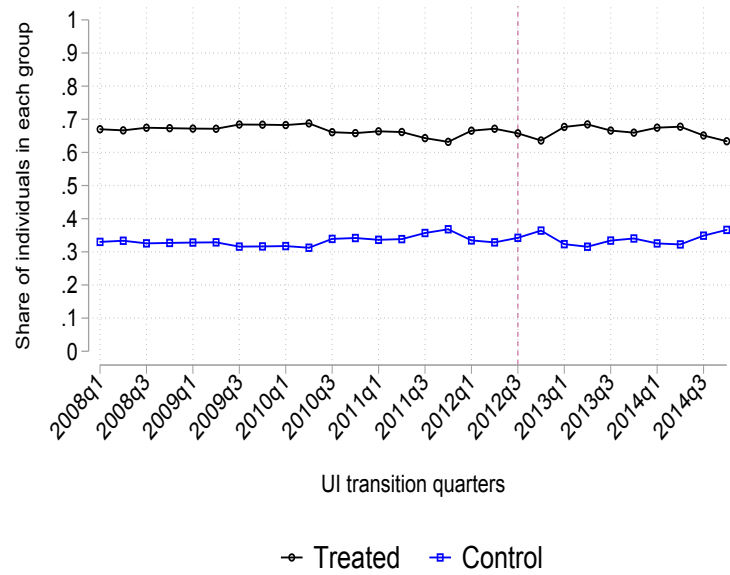
**Figure II-17: Evolution of Monthly and Daily Earnings**

*Notes:* These figures illustrate the evolution of monthly (left) and daily (right) earnings in Spain for Employment, according to the Social Security records and the tax files. The sample is restricted to individuals who are older than 18.

*Source:* Authors' calculations based on [MCVL](#) 2005-2017 data.

## II.1.2 Empirical Analysis Figures

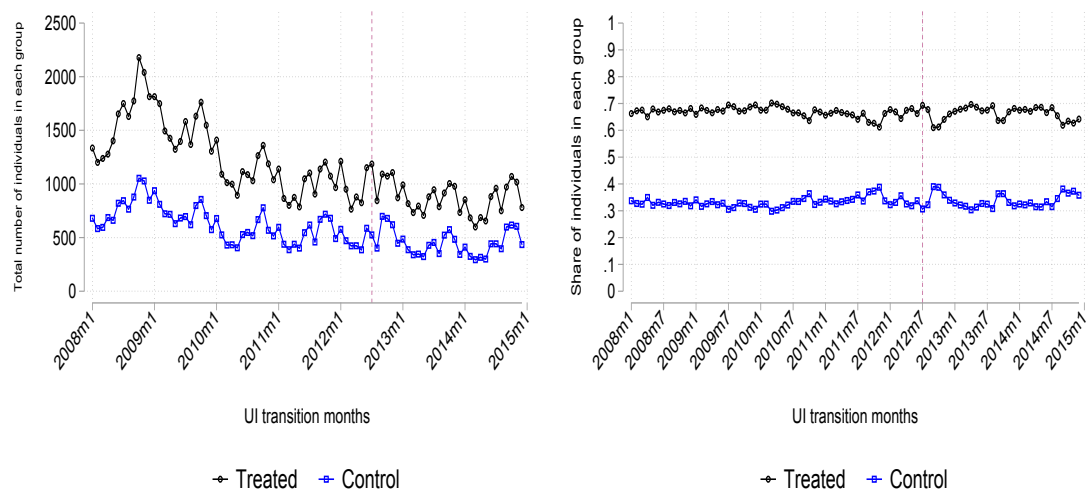
**Figure II-18: UI Transitions per Quarter (Percentage)**



*Notes:* This figure shows the quarterly transitions to **UI**, in absolute and relative terms, for both the control and the treatment group. The sample is restricted to individuals who are 20 to 52 years old, with an **UI** entitlement length of at least 120 days. It includes individuals who enter their **UI** benefit spell between 2008 and 2014.

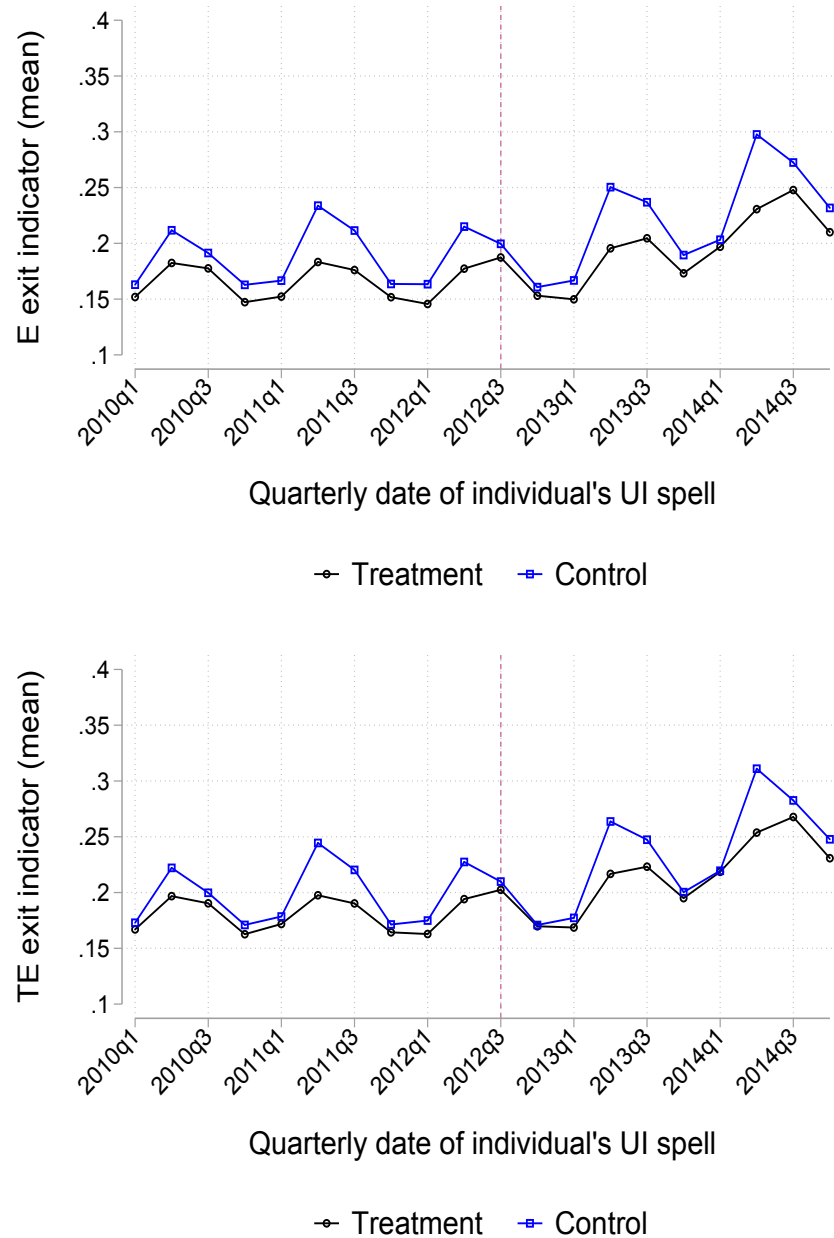
*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Figure II-19: UI Transitions per Month (Total Numbers and Percentage)**



*Notes:* These figures show the monthly transitions to **UI**, in absolute (left) and relative (right) terms, for both the control and the treatment group. The sample is restricted to individuals who are 20 to 52 years old and who have an **UI** entitlement length of at least 120 days. It includes individuals who enter their **UI** benefit spell between 2008 and 2014.

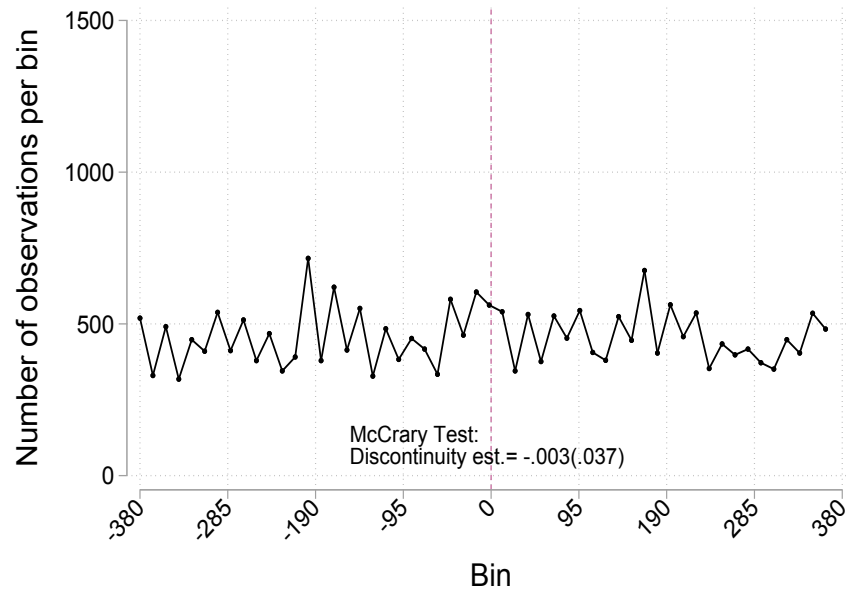
*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Figure II-20: Parallel Trends Check for Employment and Total Employment**

*Notes:* These figures show quarterly average probabilities of exiting from unemployment (**UI**) into employment (E) and total employment (TE) in the period between Q1/2010 and Q4/2014. The sample is restricted to individuals who are 20 to 52 years old and who have an **UI** entitlement length of at least 120 days. The reform quarter is highlighted with a red dashed line. These figures illustrate that the common trend assumption holds, i.e. that there appear to exist parallel trends in both treatment and control group before the reform.

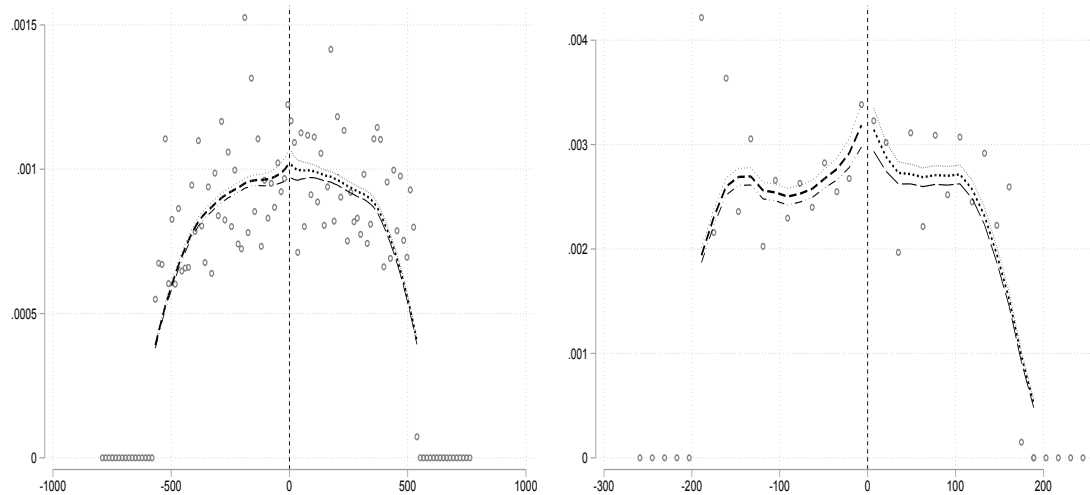
*Source:* Authors' calculations based on the **MCVL** 2005-2017 data.



**Figure II-21:** Distribution of Observations per Bin for the McCrary Test (Medium Sample)

*Notes:* The sample consists of all individuals who are 35 to 52 years old, with more than 180 days of **UI** benefit entitlement. We consider only entries into **UI** between 1 July 2011 and 31 July 2013 (medium sample). Each bin covers 14 days.

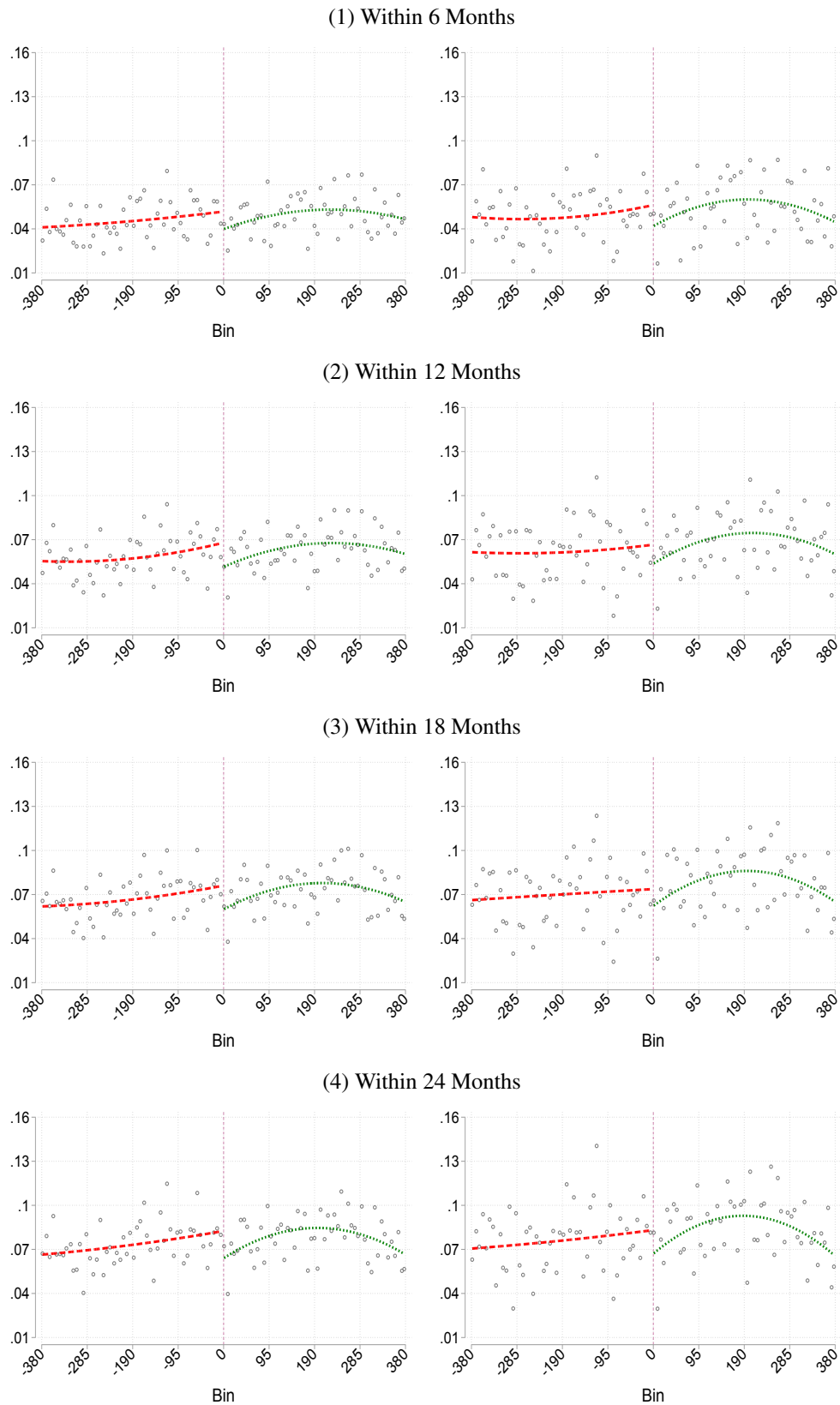
*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Figure II-22:** McCrary Test for Individuals Entering their UI Spell (Large/Small Sample)

*Notes:* These figures illustrate the McCrary tests for individuals entering their **UI** spell within 540 (large sample) and 180 (small sample) days distance from the cutoff, respectively. The large sample (left) consists of individuals entering their **UI** spell between January 2011 and December 2013. The small sample (right) only considers entries into the **UI** system in 2012. In both samples, we consider individuals who are 35 to 52 years old and who have more than 180 days of **UI** benefit entitlement. The discontinuity estimates (log differences in height) are 0.0005 (0.0352) for the large sample, and -0.0134 (0.0582) for the small sample. Standard errors are given in parentheses.

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

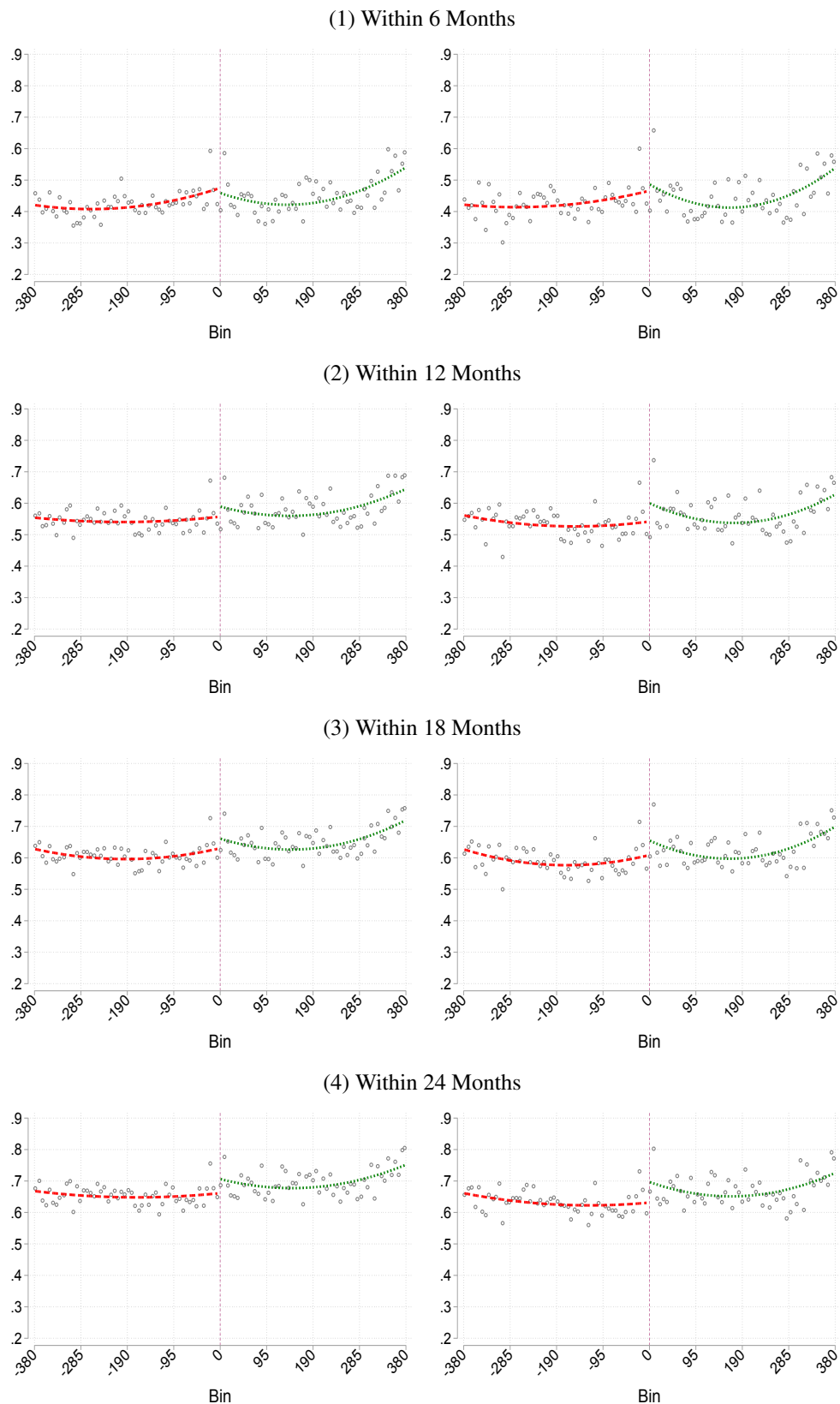
**Figure II-23: RDD Reform Effect on Self-Employment Probabilities by Age Restrictions (20-52 left vs. 35-52 right)**



Notes: These figures illustrate RDD results for our quadratic prediction plots of self-employment probabilities. The sample is restricted to individuals entitled to more than 180 days of UI benefits who are 20 to 52 years old in the left, and 35 to 52 years old in the right figure panels. We only consider individuals whose UI benefit spell starts between 1 July 2011 and 31 July 2013 (medium sample).

Source: Authors' calculations based on MCVL 2005-2017 data.

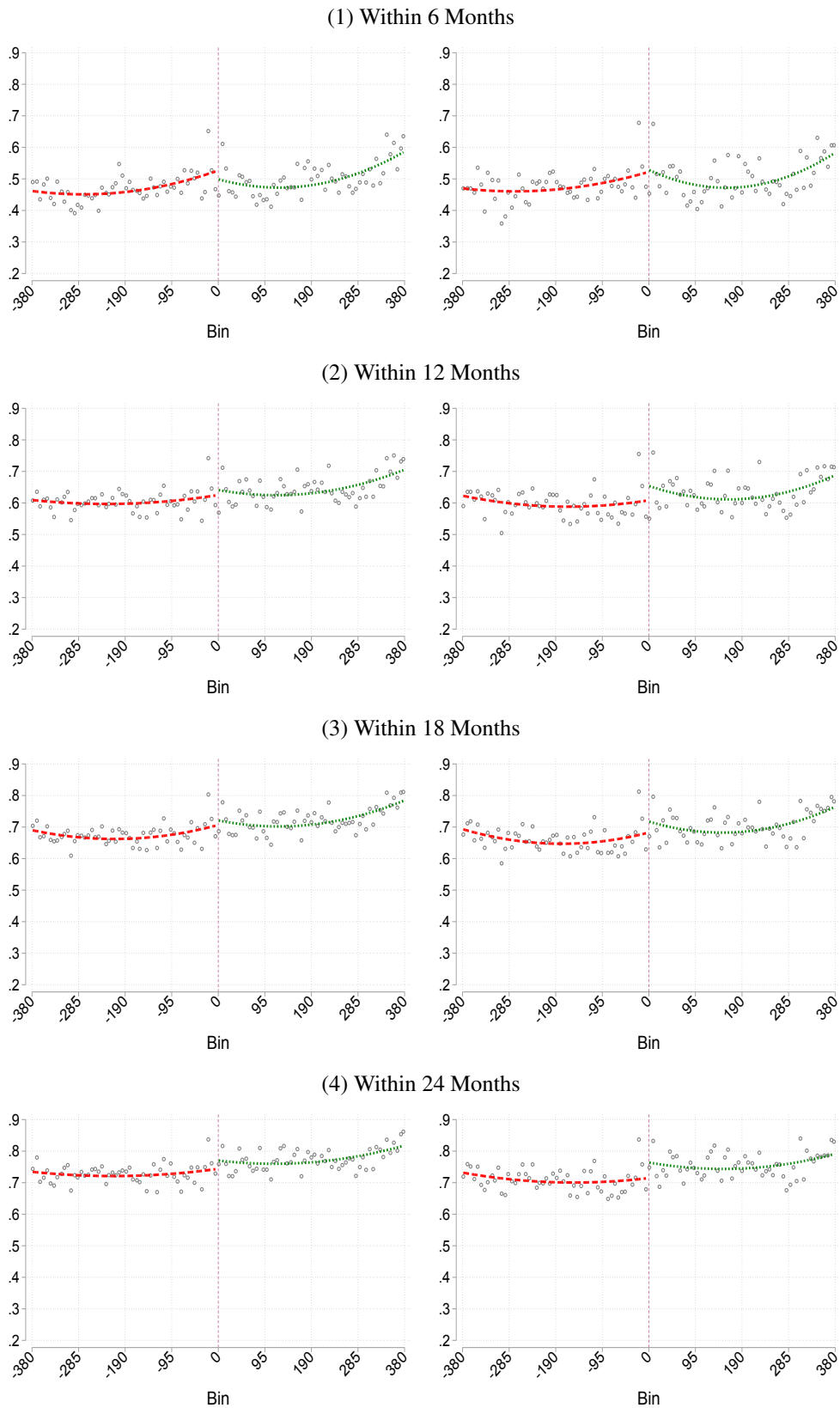
**Figure II-24:** RDD Reform Effect on Employment Probabilities by Age Restrictions (20-52 left vs. 35-52 right)



*Notes:* These figures illustrate RDD results for our quadratic prediction plots of employment probabilities. The sample is restricted to individuals entitled to more than 180 days of UI benefits who are 20 to 52 years old in the left, and 35 to 52 years old in the right panel figures. We only consider individuals whose UI benefit spell starts between 1 July 2011 and 31 July 2013 (medium sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Figure II-25: RDD Reform Effect on Total Employment Probabilities by Age Restrictions (20-52 left vs. 35-52 right)**



Notes: These figures illustrate RDD results for our quadratic prediction plots of total employment probabilities. The sample is restricted to individuals entitled to more than 180 days of UI benefits who are 20 to 52 years old in the left, and 35 to 52 years old in the right figure panels. We only consider individuals whose UI benefit spell starts between 1 July 2011 and 31 July 2013 (medium sample).

Source: Authors' calculations based on MCVL 2005-2017 data.

## II.2 Appendix: Supplementary Tables

**Table II.1:** Personal Characteristics

	Mean	Std. Dev.
Female	0.46	(0.50)
Age at entry into spell	41.11	(12.64)
Immigrant dummy (nationality)	0.18	(0.38)
Kids	0.40	(0.49)
Less than secondary education	0.58	(0.49)
Secondary education	0.25	(0.43)
University education	0.17	(0.38)
Agriculture	0.03	(0.18)
Industry	0.08	(0.27)
Construction	0.05	(0.22)
All Services and others	0.52	(0.50)
SE experience in months	30.46	(80.29)
WE experience in months	143.62	(136.41)
SE tenure in months	24.48	(71.64)
WE tenure in months	46.29	(76.85)
Observations	1,307,568	

*Notes:* This table presents the mean and standard deviation for some main personal characteristics in the **MCVL** sample between 2005 and 2017. Each observation corresponds to one individual. Time-varying characteristics refer to the last spell of an individual. The sample is restricted to individuals older than 18.

*Source:* Authors' calculations based on the **MCVL** 2005-2017 data.

**Table II.2:** Personal Characteristics: Self-Employed and Employed

	SELF-EMPLOYMENT		EMPLOYMENT	
	Mean	Std. Dev.	Mean	Std. Dev.
Female	0.36	0.48	0.48	0.50
Age at entry into spell	43.49	11.54	39.29	11.76
Immigrant dummy	0.12	0.33	0.18	0.38
Kids	0.42	0.49	0.43	0.50
Less than secondary education	0.60	0.49	0.53	0.50
Secondary education	0.25	0.44	0.26	0.44
University education	0.15	0.36	0.21	0.40
Agriculture	0.09	0.29	0.04	0.19
Industry	0.07	0.26	0.12	0.32
Construction	0.12	0.33	0.06	0.24
All Services and Others	0.68	0.46	0.72	0.45
SE experience in months	164.75	132.03	7.67	31.95
WE experience in months	73.04	85.19	166.08	138.34
SE tenure in months	138.63	129.47	5.52	25.81
WE tenure in months	24.27	45.90	59.20	84.29
Part-time contract			0.20	0.40
Temporary contract			0.40	0.49
Observations	166,808		788,439	

*Notes:* This table presents the mean and the standard deviation for some main personal characteristics. We distinguish between self-employed and employed workers. Time-varying characteristics refer to the last spell of each individual. The information refers to the sample between 2005 and 2017, which is restricted to individuals who are older than 18.

*Source:* Authors' own calculations based on the **MCVL** data.

**Table II.3:** Minimum and Maximum UI Benefit Amount (valid 2010-2016)

Dependent Children	Minimum	Maximum
0	80% IPREM + $1/6 \cdot$ (monthly benefit) [497.01€]	175% IPREM [1,087.21€]
1	107% IPREM + $1/6 \cdot$ (monthly benefit) [664.75€]	200% IPREM [1,242.52€]
$\geq 2$	107% IPREM + $1/6 \cdot$ (monthly benefit) [664.75€]	225% IPREM [1,397.84€]

*Notes:* This table summarizes the computation of the legal maximum and minimum benefit amounts. Note that these limits depend on the family responsibilities (number of dependent children or descendants) and the value of the IPREM index in a given year. We present the amounts for the period 2010-2016, when the IPREM index remained unchanged at 532.51 Euro per month.

*Source:* Authors' own illustration based on the [SEPE \(2019\)](#).

**Table II.4:** Summary Statistics (age group 20-52)

Variable	Mean	Std. Dev.	Min.	Max.	N
treated	0.66	0.47	0	1	51,867
post	0.46	0.5	0	1	51,867
treated_post	0.31	0.46	0	1	51,867
female	0.5	0.5	0	1	51,867
age_entry	33.53	7.95	20	51.92	51,867
immigrant_nat	0.18	0.38	0	1	51,867
children	0.53	0.5	0	1	51,867
university	0.15	0.36	0	1	51,867
med_educ	0.24	0.43	0	1	51,867
low_educ	0.61	0.49	0	1	51,867
low_skill	0.59	0.49	0	1	51,867
med_skill	0.3	0.46	0	1	51,867
high_skill	0.1	0.3	0	1	51,867
sec_agri	0.03	0.17	0	1	50,823
sec_industry	0.1	0.3	0	1	50,823
sec_construction	0.12	0.33	0	1	50,823
sec_service	0.36	0.48	0	1	50,823
WE_exper	2,104.6	1,297.92	364	8,689	51,867
SE_exper	238.09	795.71	0	11,030	51,867
UI_entitlement	340.41	199.66	120	720	51,867
UI_dur	160.75	160.5	1	773	51,867
UE_dur	304.05	369.55	1	2,507	51,867
gdp_gr_entry	-0.49	0.37	-0.98	0.29	51,867

*Notes:* This table presents some summary statistics for the covariates that are relevant for our estimations. We also include statistics on sector and UI entitlement. The sample corresponds to those individuals who are 20 to 52 years old and became unemployed between January 2011 and December 2013 (large sample).

*Source:* Authors' own calculations based on the [MCVL](#) data.

**Table II.5:** Summary Statistics (age group 35-52)

Variable	Mean	Std. Dev.	Min.	Max.	N
treated	0.71	0.46	0	1	20,568
post	0.47	0.5	0	1	20,568
treated_post	0.34	0.47	0	1	20,568
female	0.51	0.5	0	1	20,568
age_entry	41.88	4.60	35	51.92	20,568
immigrant_nat	0.21	0.41	0	1	20,568
children	0.63	0.48	0	1	20,568
university	0.11	0.31	0	1	20,568
med_educ	0.25	0.43	0	1	20,568
low_educ	0.64	0.48	0	1	20,568
low_skill	0.64	0.48	0	1	20,568
med_skill	0.28	0.45	0	1	20,568
high_skill	0.07	0.26	0	1	20,568
sec_agri	0.03	0.18	0	1	20,051
sec_industry	0.1	0.3	0	1	20,051
sec_construction	0.14	0.35	0	1	20,051
sec_service	0.35	0.48	0	1	20,051
WE_exper	2,927.24	1,418.56	365	8,689	20,568
SE_exper	488.23	1,154.36	0	11,030	20,568
UI_entitlement	379.45	216.04	120	720	20,568
UI_dur	181.25	187.06	1	769	20,568
UE_dur	325.59	392.79	1	2507	20,568
gdp_gr_entry	-0.48	0.37	-0.98	0.29	20,568

*Notes:* This table presents some summary statistics for the covariates that are relevant for our estimations. We also include statistics on sector and **UI** entitlement. The sample corresponds to those individuals who are 35 to 52 years old, and became unemployed between January 2011 and December 2013 (large sample).

*Source:* Authors' own calculations based on the **MCVL** data.

**Table II.6:** Mean-Comparison Tests (age group 20-52)

	PRE-REFORM			POST-REFORM		
	Mean TG	Mean CG	Mean diff.	Mean TG	Mean CG	Mean diff.
female	0.49	0.50	-0.02**	0.51	0.51	-0.00
age_entry	33.98	32.27	1.71***	34.35	32.16	2.19***
immigrant_nat	0.17	0.21	-0.05***	0.15	0.18	-0.03***
children	0.52	0.53	-0.01	0.52	0.54	-0.02***
university	0.14	0.13	0.01	0.18	0.16	0.02***
med_educ	0.25	0.23	0.01**	0.25	0.23	0.01**
low_educ	0.62	0.64	-0.02***	0.58	0.61	-0.03***
low_skill	0.59	0.63	-0.03***	0.58	0.62	-0.04***
med_skill	0.30	0.29	0.02***	0.30	0.29	0.01
high_skill	0.10	0.09	0.02***	0.12	0.09	0.04***
sec_agri	0.02	0.02	0.01**	0.04	0.03	0.01***
sec_industry	0.10	0.09	0.01***	0.10	0.09	0.01**
sec_construction	0.15	0.13	0.01***	0.11	0.09	0.02***
sec_service	0.34	0.36	-0.02***	0.36	0.41	-0.04***
WE_exper	2,316.97	1,559.66	757.31***	2,434.16	1,613.00	821.17***
SE_exper	221.98	241.42	-19.45*	241.90	268.02	-26.11**
UI_entitlement	437.12	144.04	293.08***	445.66	144.33	301.33***
UI_dur	202.92	103.84	99.08***	182.07	102.49	79.58***
UE_dur	347.69	305.97	41.72***	287.19	258.34	28.85***
gdp_gr_entry	-0.61	-0.61	-0.004	-0.33	-0.36	0.02***

*Notes:* Robust standard errors are shown in parenthesis and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table presents the results of the mean-comparison t-tests for our treatment and control groups, both before and after the reform. The sample corresponds to those individuals who are 20 to 52 years old, and became unemployed between January 2011 and December 2013 (large sample). We restrict our sample to those individuals with 120 days of UI entitlement or more.

*Source:* Authors' calculations based on the MCVL data.



**Table II.7:** Mean-Comparison Tests (age group 35-52)

	PRE-REFORM			POST-REFORM		
	Mean TG	Mean CG	Mean diff.	Mean TG	Mean CG	Mean diff.
female	0.49	0.53	-0.04***	0.51	0.52	-0.01
age_entry	41.88	41.84	0.04	41.85	41.82	0.02
immigrant_nat	0.22	0.23	-0.02*	0.18	0.20	-0.02*
children	0.62	0.65	-0.03**	0.61	0.66	-0.05***
university	0.11	0.09	0.03***	0.14	0.10	0.04***
med_educ	0.26	0.25	0.01	0.27	0.23	0.04***
low_educ	0.63	0.67	-0.04***	0.60	0.67	-0.07***
low_skill	0.65	0.68	-0.04***	0.63	0.70	-0.07***
med_skill	0.28	0.26	0.02**	0.27	0.24	0.03***
high_skill	0.07	0.06	0.01**	0.09	0.06	0.03***
sec_agri	0.03	0.02	0.01***	0.04	0.03	0.01***
sec_industry	0.10	0.09	0.02***	0.11	0.10	0.01
sec_construction	0.16	0.16	-0.00	0.12	0.12	-0.00
sec_service	0.34	0.33	0.01	0.37	0.38	-0.01
WE_exper	3,009.24	2,453.38	555.86***	3,177.00	2,643.01	533.98***
SE_exper	450.58	520.56	-69.98***	467.17	634.46	-167.29***
UI_entitlement	467.92	143.11	324.81***	485.07	143.72	341.35***
UI_dur	227.06	105.42	121.65***	207.07	102.89	104.18***
UE_dur	377.13	305.40	71.73***	319.10	263.52	55.58***
gdp_gr_entry	-0.61	-0.60	-0.01***	-0.33	-0.35	0.03**

*Notes:* Robust standard errors are shown in parenthesis and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table presents the results of the mean-comparison t-tests for our treatment and control groups, both before and after the reform. The sample corresponds to those individuals who are 35 to 52 years old, and became unemployed between January 2011 and December 2013 (large sample). We restrict our sample to those individuals with 120 days of UI entitlement or more.

*Source:* Authors' calculations based on MCVL data.

**Table II.8:** Robustness Check of UI and UE Duration Estimates (DiD)

<b>Outcome</b>	<b>Baseline</b>	<b>+ Socioec.</b>	<b>+ Macroec.</b>	<b>+ Job Charact.</b>
<i>UI Duration</i>				
Total	-15.16*** (4.645)	-15.31*** (4.646)	-15.13*** (4.472)	-14.54*** (4.667)
<i>N</i>	20,585	20,585	20,585	20,585
Employment	-15.57*** (4.692)	-15.73*** (4.698)	-15.88*** (4.399)	-15.15*** (4.648)
<i>N</i>	19,198	19,198	19,198	19,198
Self-Employment	-14.54** (6.525)	-17.87** (6.602)	-17.17** (6.804)	-18.59** (6.667)
<i>N</i>	6,844	6,844	6,844	6,844
<i>UE Duration</i>				
Total	-12.47 (10.26)	-12.82 (9.950)	-12.50 (9.663)	-13.42 (9.319)
<i>N</i>	20,585	20,585	20,585	20,585
Employment	-12.61 (10.78)	-13.22 (10.31)	-13.43 (9.752)	-14.33 (9.440)
<i>N</i>	19,198	19,198	19,198	19,198
Self-Employment	-21.58 (28.06)	-25.89 (27.82)	-24.89 (28.62)	-30.37 (26.28)
<i>N</i>	6,844	6,844	6,844	6,844

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the ATE coefficients but control for all additional sets of covariates described in the column header. We use UI/UE duration (in days) as our dependent variable. Our estimation sample includes individuals who are 35 to 52 years old and who switched into an UI spell between January 2011 and December 2013 (large sample). They have an UI entitlement duration of at least 120 days. The results for the total sample correspond to those presented in Table 2.3. The *Employment* row header corresponds to our sample that excludes individuals who transition into self-employment (it keeps those who either stay unemployed or find a job). The *Self-Employment* row header corresponds to our sample that excludes individuals who find a job after their unemployment spell (it keeps those who stay either unemployed or transition into self-employment).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Table II.9:** UI and UE Duration Average Values

Outcome	TREATMENT GROUP		CONTROL GROUP	
	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform
<i>UI</i>				
Total	222.147	204.471	105.100	102.588
Employment	226.557	208.430	105.195	102.641
Self-employment	333.456	313.830	121.542	116.453
<i>UE</i>				
Total	369.124	314.684	304.330	262.365
Employment	381.547	325.317	307.429	263.809
Self-employment	650.848	579.282	528.559	478.574

*Notes:* This table presents the **UI** and **UE** duration average values. The sample is restricted to individuals who are 35 to 52 years old, and became unemployed between January 2011 and December 2013 (large sample). The *Employment* row header corresponds to our sample that excludes individuals who transition into self-employment (it keeps those who either stay unemployed or find a job). The *Self-Employment* row header corresponds to our sample that excludes individuals who find a job after their unemployment spell (it keeps those who stay either unemployed or transition into self-employment). The *Total* row header corresponds to the sample that includes all of these cases (individuals who transition into self-employment, find a job or stay unemployed).

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Table II.10:** Difference-in-Differences (Heterogeneity Tests: UI/UE Duration)

	UI DURATION		UE DURATION	
<b>Age</b>	<b>&gt; 45</b>	<b>≤ 45</b>	<b>&gt; 45</b>	<b>≤ 45</b>
	-13.062	-15.263***	11.521	-23.850**
	(9.282)	(4.054)	(22.645)	(8.266)
<i>N</i>	5,723	14,862	5,723	14,862
<b>Children</b>	<b>With</b>	<b>Without</b>	<b>With</b>	<b>Without</b>
	-13.865***	-14.118**	-10.379	-16.536
	(4.755)	(6.043)	(13.232)	(13.716)
<i>N</i>	12,893	7,692	12,893	7,692
<b>Gender</b>	<b>Female</b>	<b>Male</b>	<b>Female</b>	<b>Male</b>
	-7.728	-20.526***	-12.081	-11.547
	(6.672)	(6.662)	(9.972)	(19.776)
<i>N</i>	10,475	10,110	10,475	10,110
<b>Contract</b>	<b>Permanent</b>	<b>Temporary</b>	<b>Permanent</b>	<b>Temporary</b>
	-9.981	-18.474***	-20.475	-22.684
	(8.842)	(3.863)	(16.211)	(15.711)
<i>N</i>	7,537	13,048	7,537	13,048
<b>Employer</b>	<b>Public</b>	<b>Private</b>	<b>Public</b>	<b>Private</b>
	-10.458	-15.895**	-17.743	-15.929
	(11.968)	(5.543)	(49.913)	(12.017)
<i>N</i>	2,186	17,825	2,186	17,825

*Notes:* Region-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the **ATE** coefficients but control for all covariates using our main specification and **UI/UE** duration as dependent variables. The sample includes all individuals who are 35 to 52 years old, who have an **UI** entitlement larger than 120 days, and who became unemployed between January 2011 and December 2013 (large sample).

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Table II.11:** Regression Discontinuity Robustness (medium sample)

<b>Outcome</b>	<b>Baseline</b>	<b>+Socioeconomic</b>	<b>+Macroeconomic</b>	<b>+Job Charac.</b>
<i>Self-Employment</i>				
6 months	-0.014 (0.009)	-0.016* (0.009)	-0.023* (0.014)	-0.024* (0.015)
12 months	-0.013 (0.010)	-0.016 (0.010)	-0.026* (0.015)	-0.028* (0.016)
18 months	-0.013 (0.011)	-0.016 (0.011)	-0.027* (0.016)	-0.029* (0.017)
24 months	-0.017 (0.011)	-0.021* (0.011)	-0.031** (0.015)	-0.033** (0.016)
<i>Employment</i>				
6 months	0.037 (0.029)	0.034 (0.028)	0.067 (0.044)	0.064 (0.040)
12 months	0.068** (0.027)	0.067** (0.027)	0.067* (0.040)	0.066* (0.037)
18 months	0.052** (0.024)	0.052** (0.024)	0.055 (0.035)	0.054* (0.032)
24 months	0.066*** (0.023)	0.065*** (0.023)	0.065** (0.032)	0.064** (0.030)
<i>Total Employment</i>				
6 months	0.024 (0.027)	0.018 (0.026)	0.044 (0.041)	0.040 (0.036)
12 months	0.054** (0.025)	0.051** (0.025)	0.042 (0.036)	0.038 (0.032)
18 months	0.039* (0.022)	0.036 (0.022)	0.028 (0.030)	0.025 (0.028)
24 months	0.049** (0.020)	0.045** (0.020)	0.033 (0.025)	0.030 (0.023)
<i>Unemployment Duration</i>				
UI	-33.834*** (12.826)	-34.917*** (12.758)	-27.796 (18.603)	-25.235 (15.991)
UE	-43.611** (18.498)	-43.923** (18.497)	-42.433 (26.105)	-39.108* (23.598)
<i>N</i>	24,961	24,961	24,961	24,961

*Notes:* UI entry-date-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the local ATE coefficients but control for all additional sets of covariates described in the column header. Our estimation sample includes individuals who are 35 to 52 years old, and entitled to more than 180 days of UI benefits, who entered their UI spell between 1 July 2011 and 31 July 2013 (medium sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Table II.12:** Regression Discontinuity Robustness (small sample)

<b>Outcome</b>	<b>Baseline</b>	<b>+Socioeconomic</b>	<b>+Macroeconomic</b>	<b>+Job Charac.</b>
<i>Self-Employment</i>				
6 months	-0.012 (0.013)	-0.019 (0.013)	-0.027 (0.018)	-0.031 (0.019)
12 months	-0.013 (0.015)	-0.020 (0.014)	-0.033* (0.018)	-0.037* (0.020)
18 months	-0.015 (0.016)	-0.024 (0.016)	-0.039** (0.019)	-0.043** (0.021)
24 months	-0.018 (0.015)	-0.028* (0.015)	-0.041** (0.019)	-0.045** (0.020)
<i>Employment</i>				
6 months	0.066 (0.045)	0.062 (0.043)	0.104** (0.052)	0.097** (0.048)
12 months	0.036 (0.042)	0.035 (0.042)	0.081* (0.049)	0.076* (0.046)
18 months	0.041 (0.036)	0.040 (0.036)	0.077* (0.043)	0.074* (0.041)
24 months	0.042 (0.033)	0.039 (0.033)	0.089** (0.039)	0.087** (0.038)
<i>Total Employment</i>				
6 months	0.054 (0.041)	0.043 (0.040)	0.076 (0.052)	0.067 (0.047)
12 months	0.024 (0.037)	0.015 (0.037)	0.048 (0.048)	0.039 (0.044)
18 months	0.027 (0.031)	0.017 (0.031)	0.038 (0.041)	0.030 (0.039)
24 months	0.023 (0.026)	0.011 (0.026)	0.047 (0.032)	0.042 (0.030)
<i>Unemployment Duration</i>				
UI	-19.643 (18.524)	-23.124 (18.441)	-38.462 (24.838)	-33.166 (22.226)
UE	-27.898 (26.334)	-29.073 (26.502)	-61.428* (35.861)	-53.811 (33.228)
<i>N</i>	12,415	12,415	12,415	12,415

*Notes:* UI entry-date-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the local ATE coefficients but control for all additional sets of covariates described in the column header. Our estimation sample includes individuals who are 35 to 52 years old, and entitled to more than 180 days of UI benefits, who entered their UI spell in 2012 (small sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Table II.13:** Small Sample Results (RDD)

(a) SE and E			(b) TE and UI/UE Duration		
<b>Outcome</b>	<b>20-52</b>	<b>35-52</b>	<b>Outcome</b>	<b>20-52</b>	<b>35-52</b>
<i>Self-employment</i>			<i>Total Employment</i>		
6 months	-0.020* (0.012)	-0.031 (0.019)	6 months	0.039 (0.037)	0.067 (0.047)
12 months	-0.030** (0.014)	-0.037* (0.020)	12 months	0.026 (0.035)	0.039 (0.044)
18 months	-0.033** (0.015)	-0.043** (0.021)	18 months	0.019 (0.029)	0.030 (0.039)
24 months	-0.034** (0.015)	-0.045** (0.020)	24 months	0.025 (0.023)	0.042 (0.030)
<i>Employment</i>			<i>Unemployment Duration</i>		
6 months	0.059 (0.038)	0.097** (0.048)	UI	-23.994 (15.438)	-33.166 (22.226)
12 months	0.055 (0.036)	0.076* (0.046)	UE	-39.327* (21.974)	-53.811 (33.228)
18 months	0.052 (0.033)	0.074* (0.041)			
24 months	0.059** (0.029)	0.087** (0.038)			
<i>N</i>	22,771	12,415	<i>N</i>	22,771	12,415

*Notes:* UI entry-date-clustered standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. We only report the local ATE coefficients but control for all covariates using our main specification. Results in the first column are based on individuals who are 20 to 52 years old. The second column refers to individuals who are 35 to 52. Both samples are restricted to individuals entitled to more than 180 days of UI benefits who entered their UI spell in 2012 (small sample).

*Source:* Authors' calculations based on MCVL 2005-2017 data.

**Table II.14:** UI and UE Duration Elasticities from RDD

Outcome	Baseline	+ Socioec.	+ Macroec.	+ Job Charact.
<i>UI</i>				
Total	0.577**	0.568**	0.463	0.419
Employment	0.664**	0.652**	0.545	0.498
Self-employment	0.375	0.305	0.139	0.108
<i>UE</i>				
Total	0.336	0.320	0.528	0.495*
Employment	0.411	0.394	0.626*	0.589**
Self-employment	0.090	0.077	0.210	0.175

*Notes:* Significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table presents the **UI** and **UE** duration elasticities, computed from the **RDD** estimation results. The elasticity  $\eta$  is computed according to the following formula:  $\eta = \frac{\Delta(\text{duration})}{\Delta(RR)}$ . In other words, we calculate the elasticity based on the percentage variation in **UI** or **UE** duration (with respect to the average duration before the reform) divided by the variation in the replacement rates due to the reform (approx. 16.67%). The sample includes all individuals who are 35 to 52 years old, with an **UI** entitlement duration of at least 180 days, who became unemployed between 1 July 2011 and 31 July 2013 (medium sample). The results for the *Total* row header are based on the total sample used in **Table 2.3**. The *Employment* header corresponds to our sample that excludes individuals who transition into self-employment (it keeps those who either stay unemployed or find a job). The *Self-Employment* header corresponds to our sample that excludes individuals who find a job after their unemployment spell (it keeps those who stay either unemployed or transition into self-employment).

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.

**Table II.15:** Mean Comparison Test for Sectors of Self-Employed Workers

	Pre UE Mean	Post UE Mean	Difference (Std. Error)
Agriculture	0.008	0.014	0.007 (0.005)
Industry	0.102	0.050	-0.052*** (0.010)
Construction	0.163	0.136	-0.028*** (0.010)
Services	0.405	0.493	0.088*** (0.016)
<i>N</i>	1,040	1,040	

*Notes:* Robust standard errors are shown in parenthesis and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. The sample consists of individuals who are 35 to 52 years old, with more than six month of **UI** entitlement (treatment group), who became unemployed between 1 January 2011 and 31 December 2013 (large sample) and transitioned into self-employment within 24 months right after their unemployment spell. We compare the pre-displacement activity sector and the self-employment sector right after unemployment.

*Source:* Authors' calculations based on **MCVL** 2005-2017 data.



## II.3 Appendix: Data and Variables

### II.3.1 MCVL Dataset

#### MCVL - Overview

*Muestra Continua de Vidas Laborales (MCVL)* means Continuous Working Lives Sample. It can be used to extract a monthly panel of administrative microeconomic data. Starting from the year 2004, *MCVL* has been released every year by Spain's Dirección General de Ordenación de la Seguridad Social (DGOSS), with 2017 as the latest edition. It contains social security data of a 4% non-stratified random sample of the population. Any individual who is working, receiving unemployment benefits, or receiving a pension in Spain could be in this sample.<sup>40</sup>

The *MCVL* consists of two versions. The version *Sin Datos Fiscales (SDF)* includes social security data without income tax records. Each edition provides data of contribution bases from which the real labor earnings can be inferred for most individuals. However, these real earnings are top- and bottom coded. In the version *Con Datos Fiscales (CDF)*, income tax records data is added, which provides information on each job and the uncensored real earnings separately. The data files contained in each edition can be merged via the person ID which is maintained across *MCVL* editions. Even though the person ID used in SDF differs from the one used in CDF, observations are obtained from the same sample. Each *MCVL* edition comprises the complete labor market history of the individuals in the sample from 1953 until the respective year of the *MCVL* wave, although earnings data are available only since 1980. Combining the editions is useful to optimize representativeness over time, since it allows to detect all individuals who are added because they have been registered with the Social Security, even though they may have been missing in one *MCVL* wave due to administrative mistakes. Thus, linking the *MCVL* editions allows to fill gaps in the affiliations with the Social Security and to update variables which are only updated when a new *MCVL* wave is produced (e.g. residence).

The *MCVL* provides not only monthly data on labor income and (un-)employment spells but also information on individual characteristics (gender, age, education, nationality, occupation, etc.), working time, and employers' characteristics (firm size, firm sector, etc.). Additionally, experience levels can be computed.

Due to space limitation this appendix provides a brief overview of our extensive data work referring to more detailed data documentations that allow a replication of our work. Moreover, we think that our data and variable documentation can prove to be useful for other researchers who intend to work with the *MCVL* data. Our data documentations are available upon request.

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<sup>40</sup>Note that in this working paper, we do not consider pension data and only partially use taxable income data.

## Data Sources

For more information on the Spanish social security data and the availability of these datasets, the reader is recommended to refer to the Dirección General de Ordenación de la Seguridad Social.

To be able to work with the **MCVL** data, one has to apply for getting access to the data which is described in details on <http://www.seg-social.es/wps/portal/wss/internet/EstadisticasPresupuestosEstudios/Estadisticas/EST211/1459>.

We created an overview document that lists all variables that are contained in each of the **MCVL** original datasets (2005-2017): “**Documentation of MCVL Variables and Labels**”.

### II.3.2 Data Construction

#### From Raw to Master Data

We refer to our first part of our data documentation “**Documentation I: From Original Data to Master Data**” for a detailed description of how to clean the original raw dataset from the Spanish social security authorities and construct our “*master dataset*”. The documentation is intended to provide guidance how to use our do-files to replicate our work and generate the *master dataset*. Our *master dataset* aims to include as much variables and information as possible (e.g. it keeps parallel and overlapping spells from side jobs), such that it can be used as starting point for other research projects. We created an Excel-document that provides an overview of all variables which we obtain in the *master dataset*: “**MCVL-Variables.xlsx**”.

Our code partially builds up on the replication files and data documentation for “Unemployment in Administrative data using Survey data as a benchmark” by Lafuente (2019), “Learning by working in big cities” by De La Roca & Puga (2017), and the replication files for “Relocation of the Rich: Migration in Response to Top Tax Rate Changes from Spanish Reforms” by Agrawal & Foremny (2019). In the data documentations, we cite them as reference when we follow the corresponding author’s approach, or we indicate in which way our concept differs from these key references.

#### From Master Data to Final Results

Our *analysis dataset* is restricted to the needs of this research project. Therefore, we only keep an individual’s main spells and eliminate parallel and overlapping spells from side jobs. We provide a detailed description of how we create our *analysis dataset* based on the *master dataset* in our documentation file “**Documentation II: From Master Data to Results**”. Finally, we created an Excel-document that provides an overview of all variables which we obtain in the process from transforming the *master dataset* into the *analysis dataset*: “**MCVL\_Variables\_Analysis**”.

### II.3.3 Variables Overview

#### Outcome Variables

- **Within measures of unemployment exit states:** This is a set of binary outcome variables that take a value of one if individual  $i$  becomes self-employed or (total) employed *within* a certain amount of months  $t$ . The variable takes a value of zero if the individual remains unemployed within this period. We choose intervals of 6, 12, 18, and 24 months.
- **Unemployment exit states of month  $t$ :** This is a set of binary outcome variables that take the value one if individual  $i$  exits from unemployment into the state of interest (either self-employment (SE), employment (E) or total employment (TE) - see Figure 2.3) in month  $t$ , given that we still observe this individual as being unemployed at the beginning of month  $t$ . The variable takes the value zero if the individual stays unemployed in month  $t$ . Consequently, we estimate the effects on three different outcome variables (SE, E, TE) per month  $t$ , where  $t$  reaches from month 1 to 26 of the unemployment spell.
- **Duration measures**
  - **UI spell duration:** This variable only considers the UI spell duration and excludes UA spells as well as unregistered periods of unemployment. It is measured in days.
  - **UE spell duration:** This variable measure the general unemployment (UE) spell duration (including UI spells, UA spells and unregistered periods of unemployment). It is measured in days.

#### Control Variables

All control variables are measured at the individual's UI spell entry.

- Socioeconomic characteristics
  - **Female dummy:** female (1), male (0).
  - **ln(age):** natural logarithm of the individual's age in years.
  - **Educational level:** distinguishes individuals with lower<sup>41</sup>, secondary<sup>42</sup>, and university<sup>43</sup> education.
  - **Children dummy:** with children (1), without children (0).
  - **Immigrant dummy:** immigrant<sup>44</sup> (1), native (0).

<sup>41</sup>This category includes up to Secondary Education and Basic Professional Training.

<sup>42</sup>This category includes Bachillerato, Advanced Professional Training and other intermediate diplomas.

<sup>43</sup>This category includes all university degrees, post-graduate and specialization studies.

<sup>44</sup>Based on the individual's nationality, regardless of birth country.

- Macroeconomic control variables
  - *Quarterly real GDP growth rate*: as obtained from [INE \(2018\)](#).
  - *UI entry month indicators*.
  - *Autonomous Regions indicators*: Andalusia, Aragon, Principality of Asturias, Balearic Islands, Basque Autonomous Community, Canary Islands, Cantabria, Castile and Leon, Castilla-La-Mancha, Catalonia, Extremadura, Galicia, La Rioja, Community of Madrid, Region of Murcia, Navarra and Valencian Community.
- Pre-displacement job characteristics:
  - *ln(employment experience)*: natural logarithm of the individual's number of days employed.
  - *ln(self-employment experience)*: natural logarithm of the individual's number of days self-employed.
  - *Occupational skill level*: distinguishes high<sup>45</sup>, medium<sup>46</sup>, and low<sup>47</sup> skilled workers.

## II.4 Appendix: Institutional Details

### II.4.1 Social Security System in Spain

The Spanish social security system is organised in four different contribution regimes (*schemes*). Ordinary employed individuals are registered within the *General Scheme*, but there are also special schemes for sea workers, coal mining workers and self-employed individuals (*Autonomous Scheme*). The Spanish social security system has gained in complexity over the years, and currently each of these broad *schemes* consists of several sub-schemes and special situations (artists, domestic workers, seasonal workers, etc.).

Moreover, the social security legislation has established a specific regulation of the regimes for some groups, such as civil servants, armed forces, or education and health workers. However, some reforms in the last decade have aimed at simplifying this intricate system ([Spanish Social Security 2018](#)). For instance, in 2008 self-employed individuals of the former *Special Scheme for Agriculture* were integrated into the *Autonomous Scheme*. Furthermore, the former *Special Scheme for Agriculture* and the *Special Scheme for Domestic Employees* were integrated into the *General Scheme* as of January 2012.

For information on unemployment and self-employment programs, we refer to our Online-Appendix “[Unemployment and Self-Employment: Institutional Background](#)”.

<sup>45</sup>This category includes administrative and technical managers, technical engineers and graduate assistants, as well as engineers, college graduates and senior managers.

<sup>46</sup>This category includes administrative assistants, subordinates and auxiliary workers, administrative officers, and non-graduate assistants.

<sup>47</sup>This category includes other laborers, as well as third-, second- and first-class officers and technicians.

#### II.4.2 Unemployment Insurance (UI)

The contribution period, which is used to calculate the length of **UI** benefit entitlements, excludes contributions which have already been used for previous **UI** spells. However, one can claim the remaining entitlements. If an individual's employment spell lasted for at least 12 months and, thus, she qualifies for **UI** benefits, the individual has the right to choose between the non-exhausted benefits from the last **UI** spell, and the new entitlement collected from the most recent employment spell. Obviously, not only the **UI PBD** may differ, but also the amount of old and new benefits because they are calculated from different pre-unemployment salaries. The non-selected entitlement will be lost. However, if the employment spell that followed the previous **UI** spell lasted for less than 12 months, the newly gathered entitlement is not lost. Instead, the worker can claim it as soon as the accumulated short-term employment spells reach the 12-month threshold (Alba-Ramirez et al. 2007).

In case of part-time employment, the eligibility of a worker can only be determined with respect to the contribution periods in those jobs from which she has already been dismissed. Moreover, from 29 July 2018 onward, the relevant contribution period corresponds to the time when the worker had an active affiliation, regardless of how many days in a given week she has worked and regardless of the amount of hours on each day worked. However, the regulatory base which is relevant for the **UI** benefits amount corresponds to the average of the individual's contribution bases in both the lost and ongoing part-time contracts. Note that the **UI** benefits amounts, which result from applying the replacement rate to the regulatory base, must be weighted by the corresponding part-time coefficient<sup>48</sup>. Lastly, part-time workers are not eligible for **UI** benefits if they work no more than 48 hours per month (Kyyrä et al. 2019).

#### II.4.3 Unemployment Assistance (UA)

**UA** eligibility requires one of the following circumstances: (1) **UI** benefits are exhausted and the individual has family dependents; (2) the individual received **UI** benefits for at least 12 months and is at least 45 years old; (3) the individual is ineligible for **UI** benefits because he or she contributed less than 12 months; (4) the individual is a returned emigrant; (5) the individual was released from prison; (6) the individual's disability spell ended because he or she was declared to be able to work; (7) the individual is at least 55 years old. The **UA** benefit amount is independent from the pre-displacement salary. Instead, a flat benefit amount equal to 80% of the IPREM is paid to **UA** recipients. The duration of entitlement to **UA** benefits can reach a maximum of 30 months, depending on the individual's age and family responsibilities (SEPE 2019).

Both **UI** and **UA** recipients are subject to penalties in terms of (partial) benefit loss if they commit an offense against provisions that regulate the unemployment protection. The level of a penalty depends on an offense's severity (SEPE 2019).<sup>49</sup>

<sup>48</sup>Hence, a half-day job collects only 50% of the benefits a full-day job would have generated (Kyyrä et al. 2019).

<sup>49</sup>There exist minor, serious, and very serious offences. The penalty becomes more severe, the more often an offence is committed. For very serious offences benefits are cancelled, and unduly collected benefits must be returned.

#### II.4.4 Self-Employment and Social Security in Spain

The concept of self-employment (*trabajo autónomo*) or own-account work (*trabajo por cuenta propia*) is actually a broad category which includes different types of individuals: self-employed workers, self-employed professionals and freelancers, self-employed entrepreneurs, economically dependent self-employed workers (*TRADE*), agrarian self-employed workers and some special cases. From a social security perspective, self-employed individuals (*the autonomous*) pay their social security contributions to the *Special Regime of Self-Employed Workers (RETA)*. RETA includes self-employed workers who are over 18 years old and not bound by a work contract, but also cases such as unpaid family members, book writers, TRADE workers, managers and CEOs.

The contributions paid by the self-employed worker (*cuotas*) depend on the level of social protection chosen by the individual, who determines the contribution rate as well as the desired contribution base. The self-employed individual (*the autonomous*) must choose a contribution base within existing legal bounds which are legally determined each year. The minimum and maximum basis from which *the autonomous* can choose depend on some personal and occupational characteristics: age, marital status, contribution history, gender, disability, etc. Starting from the legal minimum contribution base, *the autonomous* have to pay a higher percentage of their income as social security contributions depending on the level of protection they choose. For example, if an *autonomous* decides to be insured against the risk of “cessation of activity” (analogous to [UI](#) benefits in the *General Scheme*), she has to add 2.20% of her income to the minimum contribution base. To be also insured against “professional contingencies” (protection in case of inability to work due to work-related reasons like an accident), the *autonomous* must pay between 1.3% and 6.8% of personal incomes to the social insurance.

As of 2019, the Spanish government uniformed the RETA scheme, obliging all self-employed to pay all type of contingencies. De facto, the level of protection for self-employed was equalized to that of employees. It is noteworthy that, before this reform, only 19.7% of the self-employed had opted in to be covered for work accidents and occupational diseases ([Eurofound 2017](#)).

#### II.4.5 Institutional Background - Relevant Aspects for this Chapter

For details on all the variables in the [MCVL](#) dataset and their transformation, we refer to our data documentation, in particular to the Excel-document: [MCVL\\_Variables.xlsx](#)

For this chapter, it is relevant to note that we are able to see all self-employed individuals as they have to contribute to the social security system. However, we can only approximately infer the income of self-employed people by assuming that those making more profits may have chosen to contribute more to the social security. In the future, the reform of 2019 may allow researchers to better approximate self-employment income.

## II.4.6 Reforms

We present a brief overview of the main reforms affecting the Spanish labour market in the recent years, along with the strategies we implement in order to address each one of them throughout our empirical work.

### Unemployment Insurance

- **Introduction of the IPREM**, July 2004. The Public Indicator of Multiple Effects (*IPREM*) substitutes the minimum wage (*SMT*) as a reference for unemployment benefits and other social aids.
  - In our *analysis dataset*, we only include individuals transitioning to **UI** from 1 January 2008 to 31 December 2014.
- **Active Insertion Income**, November 2006. State subsidy for workers with special economic needs and difficulties to find a job (e.g. individuals older than 45).
  - Any person younger than 65 who fulfills the legal requirements may be eligible for this subsidy ([SEPE 2019](#)).
- **Labour Market Reform I**, September 2010. New classification of fair dismissal conditions, and in some cases reduction of severance payments from 45 to 20 days per year of employment .
  - The largest sample that we use to perform our analysis includes individuals who enter **UI** between 1 January 2011 and 31 December 2013.
- **PREPARA**, February 2011. New extraordinary subsidy as incentive to provide long-term part-time contracts to unemployed individuals younger than 30, as long as they commit to training programs.
  - We present our main results for individuals older than 35, as the baseline age restriction, as well as for individuals older than 20, as a robustness check.
- **Labour Market Reform II**, July 2012.
  - Replacement rate reduction from 60% to 50% of the regulatory base after the sixth month of **UI** benefits.
    - This is the policy change in the center of our causal analysis.
  - **UA** benefits extension until retirement for workers older than 55.
    - We exclude individuals older than 52 in our analysis.
- **Budgetary Stability**, December 2013. End of the public contributions to the severance payments of dismissed workers in the case of objective reasons in solvent firms.
  - Our main results are based on a sample that includes individuals who transition into **UI** in December 2013 at the latest, so that this reform affects individuals mostly beyond the samples used for this chapter.



## Self-employment Regulations

In general, our **UI** entry date and age restrictions account for all of these reforms, which were mainly directed at younger individuals until 2015. Some reforms affected the whole labor force in the same way, and thus do not potentially violate our regression methods.

- **Self-employed Workers Statute**, October 2007.
  - Extension of social protection for temporary sick-leave to the self-employed.
  - Definition of the role of economically-dependent self-employed workers (TRADE).
- **Cease-of-activity Benefits (CAB)**, August 2010. Introduction of CAB as a voluntary contingency linked to work accidents and professional illness contingencies. CAB amounts are based on the principle of contribution-benefits.
- **Incentives to Entrepreneurship and Job Creation**, March 2013.
  - Capitalization of **UI** benefits for young employed workers: payment of 100% of the **UI** benefits to men younger than 30 and women younger than 35 who want to become self-employed.
  - Reactivation of outstanding **UI** benefit payments after being self-employed with better conditions for workers under 30.
- **Strategy of Entrepreneurship and Youth Employment**, August 2013.
  - Flat and reduced rate of social security contributions for young self-employed workers (men under 30 and women under 35).
  - Improvement of financing for young self-employed workers.
- **Promotion of Self-employment**, October 2015. Generalization of many advantages of young self-employed workers to all individuals.
- **Further Reforms**, December 2018.
  - All voluntary contingencies become compulsory (CAB and professional contingencies).
  - CAB duration is extended up to 24 months.



## **Chapter 3**

# **Inequality of Educational Opportunities and the Role of Learning Intensity**

### 3.1 Introduction

In modern societies, the general belief that by working and studying hard everyone has a fair chance at climbing the social ladder has been central to maintaining social cohesion and political stability. However, in an era of relatively high income and wealth inequality compared to the post-war decades in most developed countries (Piketty & Zucman 2014), an increase in the number of both citizens who fear that their children may be worse off in the future (fear of *downward* mobility) and groups in society who believe that the “game is rigged” (fear of a lack of *upward* mobility) may be crucial for explaining rising political polarization. For these reasons, the reduction in social mobility<sup>1</sup> is becoming an increasingly important issue when it comes to understanding recent trends of inequality within society. As education tends to be the main vehicle for *upward* mobility, thus, it is of key policy interest to analyze educational systems in terms of equality, in particular to detect drivers of *Inequality of Opportunity* (as Chetty et al. (2020) for US colleges). Yet in times of public spending constraints, accelerating growth of scientific knowledge and economic competition within OECD countries, educational policies have still shifted their attention onto how to make a country’s educational system more efficient (Machin 2014). In fact, recent reforms have started to focus on compressing educational processes, that is, on increasing *learning intensity*.

This third chapter of my doctoral thesis contributes to the issue of how the trend in intensification of education may explain decreased social mobility by analyzing the question of how increasing *learning intensity* affects *Inequality of Educational Opportunity* (IEOp). Thus, I shift focus onto the distributional concerns and the potential unintended consequences of compressing educational processes for social mobility. If, for instance, higher intensity made it harder to learn the curriculum through schooling alone, educational opportunities could become more dependent on a student’s parental support resources. In this context, I will adopt the concept explained by Roemer & Trannoy (2015) which states that society has achieved *Equality of Opportunity* if what individuals achieve with respect to a desirable objective is determined by their *efforts* (e.g. how hard they study), instead of by *circumstances* that are beyond an individual’s control (e.g. gender). IEOp<sup>2</sup> is hence defined as inequality in the distribution of educational outcomes that can only be attributed to *circumstances* through either their direct or indirect (via changing *efforts*) impact on outcomes. It is a relative measure of educational mobility. This chapter is among the first to provide an analysis of *Inequality of Educational Opportunity* (IEOp) in a quasi-experimental setting that is going beyond its pure measurement.

<sup>1</sup>For instance, Chetty et al. (2017) provide evidence for falling absolute income mobility. OECD (Organization of Economic Cooperation and Development) data from 2012 confirm low absolute educational mobility. In particular, Germany reaches only below average social mobility rates in terms of the percentage of 25-64 year-old non-students whose educational attainment is higher (*upward* mobility) or lower (*downward* mobility) than that of their parents (see Graph A.4.3 in Boserup et al. (2018)).

<sup>2</sup>*Inequality of Opportunity* (IOp) and *Equality of Opportunity* (EOp) refer to the same concept, placing emphasis on either the unfair or fair part within the distribution of opportunities. Thus, if opportunities depend less on factors beyond an individual’s control but more on their *efforts*, EOp will increase and IOp will decrease. In line with Brunori et al. (2012), instead of IOp in education, I use the expression IEOp; and instead of EOp in education, *Equality of Educational Opportunity* (EEOp). In the following, I will only use IOp or IEOp for ease of interpretation.

As Ramos & Van de gaer (2016) point out, the understanding of how institutions influence IEOp is still limited. Therefore, my contribution to this issue consists in providing evidence on the role of learning intensity as a relevant policy dimension that causally affects IEOp. From a social welfare perspective, it is interesting to reveal the effects of increasing learning intensity on both academic achievement and IEOp. Pareto-improvements may be realized if more intense curricula proved to be an instrument to overcome the trade-off between educational spending and schooling outcomes.

To identify the causal effect of (increased) learning intensity on IEOp, I analyze an educational reform in Germany. During the last decade, Germany's federal states shortened secondary school for the academic track (*Gymnasium*) from nine to eight years at staggered time points between 2001 and 2008. The so called *Gymnasium-8 reform* (G-8 reform) reduced school duration by one year, but kept the curriculum unchanged for the affected (treated) student cohorts. Due to the implementation of the reform, there were two cohorts who would finish school together in the same year in which they received their university access diploma. However, one cohort entered one year earlier than the other, leading to differences in years of schooling (9 vs. 8 years). As both cohorts had to take the same final exams in the same year, treated students had less time to learn the same material, thus experiencing higher learning intensity. For that reason, the staggered introduction of the reform across federal states generates quasi-experimental variation that allows the application of a Difference-in-Differences estimation approach (DiD) to derive the causal effect of the increase in learning intensity on IEOp by comparing the respective treatment and control groups over time.

For the purpose of measuring IEOp, I use *Program for International Student Assessment (PISA)* data to construct a representative sample of students in the ninth grade. They include standardized test scores in reading, mathematics and science that are comparable across time and federal states, in contrast to grading schemes that depend on year and state. Moreover, these data contain a rich set of family background variables that allow me to define relevant *circumstances*. I also apply a new machine learning approach to cross-validate my theory-driven choice of variables. Ultimately, IEOp reflects the coefficient of determination when regressing test scores on these *circumstances* variables.

The analysis yields three main findings. First, the estimated size of IEOp, 20 to 35 percent of the variance in cognitive test scores that can be only attributed to *circumstances*, corresponds to the levels of common estimates for inequality of opportunity in income. Second, the reform-induced increase in learning intensity led to a significant rise in IEOp, by at least 10 percentage points of the explained test score variance, for affected (treated) students. Given the initial size of IEOp and the fact that this chapter's IEOp measures are lower bound estimates, this corresponds to relative increases in IEOp of at least 25 percent. Third, the results provide some evidence in favor of the existence of subject-dependent curricular flexibilities. Skills in mathematics and science tend to be more inflexible, i.e. more responsive to changes in curricular intensity, than reading competency, which is less dependent on schooling and more often trained through its usage in everyday

life. Finally, the results can be rationalized by differential compensation possibilities for higher **learning intensity** depending on parental resources, especially the capacity to provide additional tuition to support students with school work. This shows that there are important distributional concerns with respect to providing equal opportunities (compare e.g. [Andreoli et al. \(2018\)](#)) that must be taken into account when designing reforms altering the intensity of educational processes.

This chapter of my dissertation offers several contributions to the existing literature. First, I contribute to the strand of research on measuring Inequality of Opportunity (**IOp**) with respect to educational outcomes by adding empirical evidence on how Inequality of Educational Opportunity (**IEOp**) changed over time in a developed country. So far, papers dealing with **IOp** have focused on measurement issues, using income as the main outcome variable (e.g. [Almås et al. \(2011\)](#)). Concerning **IOp** in educational outcomes, most studies focus on measuring **IEOp** for developing countries (e.g. [Gamboa & Waltenberg \(2012\)](#)). The few papers on developed countries follow mostly a cross-country comparison approach using **PISA** data to achieve comparability of educational achievement measures over time and across countries (e.g. [Ferreira & Gignoux \(2013\)](#)). Instead, my study estimates **IEOp** for Germany exploiting quasi-experimental within-country variation (as [Cantoni et al. \(2017\)](#) for China). Such settings allow going beyond measuring **IEOp** to actually estimate the causal effects of specific policies on **IEOp**. For instance, some studies analyze **IEOp** in the context of reforms that changed tertiary education systems (e.g. [Brunori et al. \(2012\)](#) on Italy). They find both expanding higher education through opening more sites and reducing the length to get a first-level degree to have a positive effect on Equality of Educational Opportunity (**EEOp**). However, only a few studies investigate the impact of school reforms on **IEOp** (e.g. [Edmark et al. \(2014\)](#) for Sweden). In this chapter, I add evidence on how **IEOp** changed over time in Germany and focus on estimating the causal effect of increasing **learning intensity** on **IEOp**.

Second, this work contributes to a strand of literature analyzing educational policy reforms to identify the role of different input factors in the human capital accumulation process. Even though the **G-8 reform** shows that changing school intensity is an important consideration in educational policy-making, research on such reforms is still limited. To begin with, empirical work has analyzed the effects of variations in pure schooling quantity without considering **learning intensity**. In that context, most studies focus on reforms that increase educational participation, such as policies raising compulsory minimum duration of schooling. They usually find the returns of additional schooling on earnings to be positive (e.g. [Angrist & Krueger \(1991\)](#), [Grenet \(2013\)](#), [Aakvik et al. \(2010\)](#)). Furthermore, the impact of differences in instructional time on academic performance has been investigated. Relying on either cross-national or within-country variation in instructional time, most studies find a positive impact of additional time on standardized test scores (e.g. [Aksoy & Link \(2000\)](#), [Marcotte \(2007\)](#), [Lavy \(2015\)](#)). However, only a few studies have analyzed the impact of variations in instructional time when curricular content can be assumed to remain constant.

In this context, reforms that shortened the duration of schooling while keeping curricular content unchanged, allow for evaluating the impact of increasing **learning intensity**. For instance, analyzing a similar school reform in parts of Canada, [Krashinsky \(2014\)](#) finds only low long-term effects on wages. This suggests that increased **learning intensity** might not affect earnings permanently.<sup>3</sup> The results are in line with [Pischke \(2007\)](#), who exploits a German reform in the 1960s that changed the start of the school year to autumn by implementing two short school years. The reform led to a significant increase in the number of students repeating a grade, but only small effects on earnings persisted. Despite the resulting public controversy that has even led some federal states to reverse the reform, only a few studies have evaluated the **G-8 reform** and its effects on educational outcomes (e.g. [Büttner & Thomsen \(2015\)](#), [Huebener & Marcus \(2017\)](#)). Those studies tend to find non-significant positive effects of the reform on a student's average cognitive test scores as well as on educational outcomes, such as final marks for the university access diploma. However, the reform led to falling enrollment rates at university ([Marcus & Zambre 2019](#)).<sup>4</sup> Instead, my analysis shifts focus in the evaluation of the **G-8 reform** onto distributional concerns. This is relevant in the debate surrounding reforms to the secondary school system. In particular, it provides policy suggestions for how to design curricula that take the effect of **learning intensity** on both cognitive skill formation and on **IEOp** into account. For instance, implementing a whole-day school system may limit the role of parents for students to deal with compressed schooling.

Thirdly, my work relates to the emerging literature on finding drivers of inequality in educational outcomes that are key determinants of recent trends in lower social mobility (e.g. [Chetty et al. \(2020\)](#), [Philippis & Rossi \(2019\)](#), [Boneva & Rauh \(2018\)](#), [Rothstein \(2019\)](#)). I contribute to this strand of research by providing evidence that the so far neglected factor of **learning intensity** might be a relevant policy channel for both the effectiveness of (non-)cognitive skill formation and the importance of *circumstances* for educational outcomes. Whereas my analysis mainly focuses on exploiting a school reform to derive causal estimates on how intensified instruction affects **IEOp**, the interpretation of my results in terms of potential mechanisms complements explanations delivered by this strand of literature. Although a complete model of **learning intensity**, **IEOp** and its connection to social mobility is beyond the scope of this study, I provide evidence on which future research tackling this big picture question can base itself. This also supports the integration of **learning intensity** as a key factor into the human capital literature.

The remainder of this third chapter of my dissertation is organized as follows. **Section 3.2** illustrates the institutional background and the **G-8 reform** on which the identification strategy relies. **Section 3.3** explains how **IEOp** is measured given the data in this study. In **Section 3.4**, the empirical strategy is illustrated. **Section 3.5** provides the results with robustness checks and a discussion on the implications. **Section 3.6** concludes.

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<sup>3</sup>Whether this is true due to schooling working primarily as a signal or whether increased intensity may compensate for less schooling and maintain the human capital accumulation process, is unclear.

<sup>4</sup>For related literature which evaluates other outcomes of the **G-8 reform**, please refer to Appendix III.1.3.

## 3.2 Institutional Setting: the “G-8 Reform”

This section explains the institutional background and implementation of the **G-8 reform** which can be exploited as a quasi-experiment to analyze the role of increased **learning intensity** on **IEOp**.

### 3.2.1 Institutional Background: the German School System and Reform Debate

Like the United States, Germany has a federal structure. Education policy strictly falls under the remit of the 16 federal states (*Länder*). That being said, most features are comparable across states. School starts usually at the age of six, when students enter primary school for a period of four years. Afterwards, students enter a tripartite secondary school system, where the choice of track is determined by their previous academic performance.<sup>5</sup>

Both the shortest track of secondary school, *Hauptschule*, and the intermediary track, *Realschule*, allow graduates to pursue apprenticeship programs after a total of nine or ten years of schooling. The academic track, *Gymnasium*, which this chapter focuses on, leads to a diploma (*Abitur*) granting access to university. On average, the largest share of all students in secondary school (about 40 percent of each cohort) attended this track in the time period 2000 until 2012. Traditionally, the academic track used to last for nine years (for a total of 13 years including primary school) in West Germany. However, the former German Democratic Republic (GDR) had a different school system: All students were taught together for ten years, after which they could either follow vocational training or complete two additional years of *Gymnasium* to obtain the *Abitur*. Following reunification, most East German federal states adjusted to the West German standard, the **Gymnasium-9 model (G-9 model)**, but two states, Saxony and Thuringia, maintained the **Gymnasium-8 model (G-8 model)**.<sup>6</sup>

Then, in the early 2000s, the nine years were perceived as a competitive disadvantage for the economy, because they contributed to the relatively advanced age at which Germans entered the labor market after school and/or university. Moreover, the long duration of the academic track was criticized for hindering the creation of a more comparable, harmonized framework for tertiary education in the European Higher Education Area (EHEA). Thus, in order to adjust school duration to the average among **OECD** countries of twelve years, federal states decided to shorten the *Gymnasium* to eight years without reducing the curriculum, also known as the **Gymnasium-8 reform (G-8 reform)**.<sup>7</sup>

<sup>5</sup>Primary schools issue recommendations for each student regarding which secondary school track the student should enter (Dustmann et al. 2017). Based on a student’s performance in primary school, recommendations were binding in federal states for the time period considered in this study. An overview of the regulations on the transition from primary to secondary education for the period studied here is available on [https://www.kmk.org/fileadmin/Dateien/veroeffentlichungen\\_beschluesse/2006/2006\\_03\\_01-Uebergang-Grundschule-Sek1.pdf](https://www.kmk.org/fileadmin/Dateien/veroeffentlichungen_beschluesse/2006/2006_03_01-Uebergang-Grundschule-Sek1.pdf).

<sup>6</sup>In addition to the three different school tracks, federal states have recently started to provide a comprehensive school (*Integrierte Gesamtschule*). In comprehensive schools, students are not channeled into specific academic paths after primary school, but can graduate after 9, 10 or 13 years. However, this option played a negligible role for the considered time period (2000-2012), because during this time the vast majority of students achieving *Abitur* still attended *Gymnasium*. See Figure III-1 in Appendix III.1.1 for further details on the German education system.

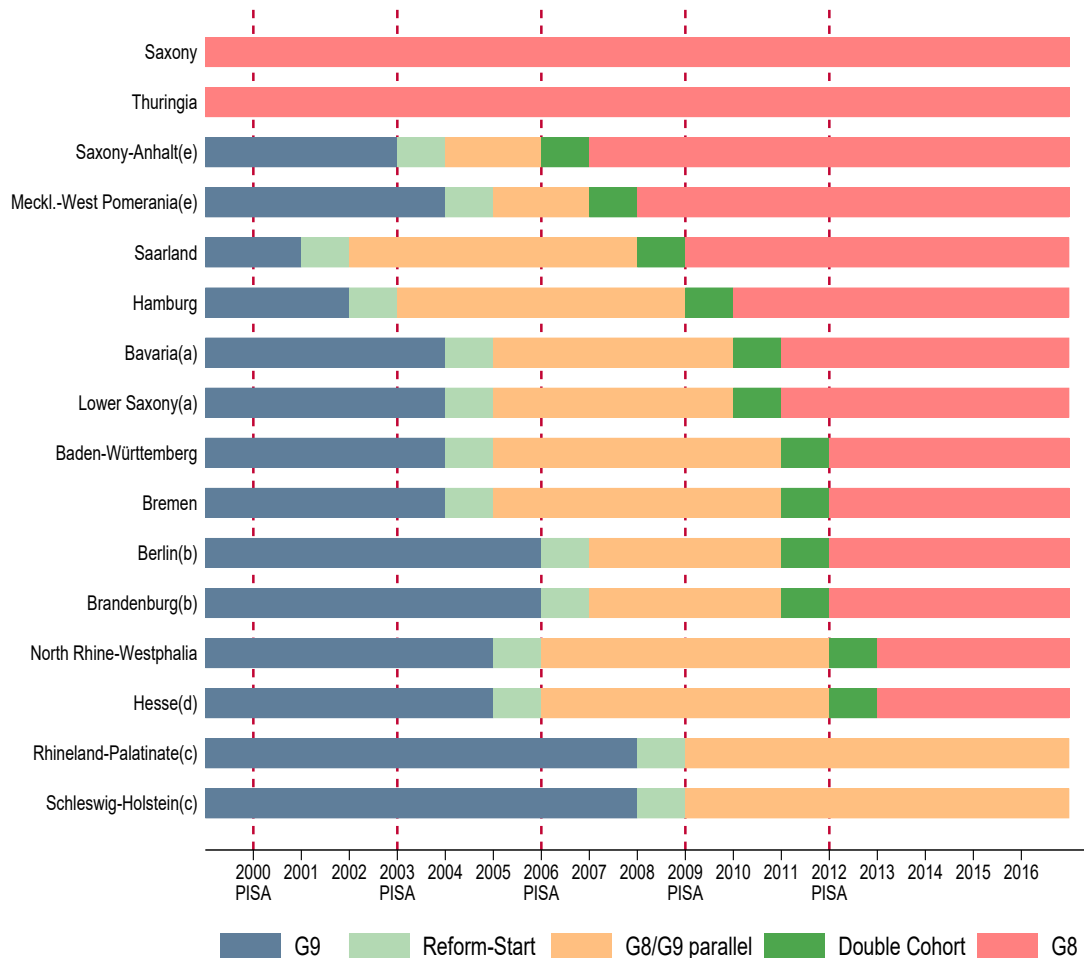
<sup>7</sup>For further arguments discussed during the reform debate, please refer to Section III.1.3 in Appendix III.1.3.



### 3.2.2 Implementation of the Reform: Increasing Learning Intensity

After 2001, all 14 federal states with a **G-9 model** shortened their academic secondary school track from nine to eight years. With the graduation of a *double cohort* consisting of both the first **G-9 model** and the last **G-9 model** student cohort that together had to pass the same final exams (*Abitur*) in the same year, the reform process took eight years to transform all grades of *Gymnasium*.

**Figure 3-1:** Implementation of the G-8 Reform across Federal States



*Notes:* This figure illustrates for each federal state whether the graduating cohort in each school year of the *Gymnasium* was in a **G-8 model**, **G-9 model**, consisted of the *double cohort* or whether due to the reform implementation process both models existed parallel with younger grades already in a G-8 and older ones still in a **G-9 model**.

*Notes on some states:*

- In Bavaria and Lower Saxony, the 5<sup>th</sup> and 6<sup>th</sup> grades were allocated into the **G-8 model** in the same school year. However, the 9<sup>th</sup> graders in 2009 were affected by the reform from the 5<sup>th</sup> grade onward.
- Berlin and Brandenburg, where primary school lasts six years, introduced the reform for 7<sup>th</sup> grade onward.
- Rhineland-Palatinate and Schleswig-Holstein planned to introduce the **G-8 reform** for school year 2008/09 to be completed by 2015/16. At the end, both kept the **G-9 model** for all grades and over all PISA waves considered.
- Hesse introduced the reform over 3 years: the “main” *double cohort* covering 60% of schools is shown.
- Mecklenburg-West Pomerania and Saxony-Anhalt introduced the reform directly for the 9<sup>th</sup> grade onward.

*Source:* Based on facts as shown in Table 3.2 and the regulations explained in Table III.1 in Appendix III.1.2. This figure corresponds to the geographical maps illustrating the implementation of the reform across time and space in Figure III-2 in Appendix III.1.1.

For the purpose of this chapter, two features of the reform are particularly important. First, as shown in [Figure 3-1](#) not all federal states started the reform process at the same time. Some of them began in school year 2001/2002, whereas others waited until school year 2008/2009, implying that the resulting *double cohorts* graduated between 2006/2007 and 2015/2016.

Second, although the academic track was reduced by one school year, the curricular content remained at the original level. In fact, education ministers decided that standards for the university access diploma (*Abitur*) were not to be lowered in response to the reform. Thus, the minimum number of 265 instruction hours for the sum of weekly lessons per school year over all grade levels was maintained, and students still had to pass the same total number of lessons before they could graduate from the *Gymnasium* (KMK 2016). This should ensure comparable nationwide standards for university access diplomas, despite the differences in school duration. Adding more content to the last two years of *Gymnasium* was perceived to be difficult, because the first *G-8 model* and the last *G-9 model* cohort had to complete those grades together. Only marks during the final two years and marks in the *Abitur* exam count towards the university access GPA. Therefore, school authorities chose to focus the compression on the first years of *Gymnasium*, squeezing the material originally taught in the seven years during grades 5 to 11 into the six years during grades 5 to 10. Thus, students in the *G-8 model* were supposed to enter the final two years of *Gymnasium* as if they had completed the original 11<sup>th</sup> grade. To keep the required total minimum weekly lessons unchanged for the new *G-8 model*, instructional time increased by about two hours a week per year during grades 5-10 for *G-8 model* students compared to previous cohorts in the *G-9 model*.<sup>8</sup>

However, the total loss in time of one school year was not fully compensated by additional instructional time per week: in order to limit the amount of afternoon schooling in 5<sup>th</sup> and 6<sup>th</sup> grade, hours originally planned for revision (beyond the minimum required) were dropped, and instead used to already teach new curricular content at an earlier point in time compared to the *G-9 model*. Therefore, it is plausible to assume that total curricular content was not reduced for the first student cohorts affected by the *G-8 reform*, that are in the focus of this study, in any of the federal states. As curricular content in the *G-8 model* began to change in the years after 2012 (cf. [Table III.1](#) in [Appendix III.1.2](#)), this assumption would not necessarily hold for later *G-8 model* cohorts. But by using data of ninth graders tested in 2012 or before, I focus on the very first cohorts affected by the reform. Thus, these later changes do not affect the analysis.

In conclusion, the *G-8 reform* exogenously led to a considerable increase in *learning intensity* over the first few years of the *Gymnasium*, that is, the amount of material covered per week increased for each grade level (excluding the final two grade levels).

<sup>8</sup>However, this is only an approximation for an average student; the exact changes depend on the federal state. Huebener et al. (2017) have collected binding timetable regulations for each federal state and show the changes in the distribution of average weekly instruction hours. This confirms the interpretation of the *G-8 reform*: on average hours per grade increased by about 2 hours a week, i.e. by about 8-10% of weekly lessons per year during grades 5-10.



### 3.3 Data and Measuring Inequality of Educational Opportunity

In this section, first, I focus on which specific **PISA** data are used for my analysis.<sup>9</sup> Second, I explain how one can measure **IEOp**, the main outcome variable, based on the related literature and the educational data available for the main test domains in mathematics, reading and science. Third, I provide some descriptive analysis on the *circumstances* variables defined for this chapter.

#### 3.3.1 PISA Data

For Germany, two types of **PISA** test data are available, the version conducted for international comparisons (**PISA-I**) and a national extension (**PISA-E**). The **PISA-I** data result from students who take the test on the same day and are selected in a two-stage sampling procedure. In the first stage, schools from the 16 federal states of Germany are randomly selected. In the second stage, for each school, about 25 students of age 15 are randomly taken for the test (*age-based* sample); additionally, within selected schools, two classes of ninth graders with a minimum of 25 students are randomly chosen (*grade-based* sample). In total, the *grade-based* **PISA-I** sample consists of about 10,000 students from about 225 schools (Table III.2 in Appendix III.1.1). Thus, its sample size is about twice as large as that of the *age-based* sample. While comparisons across countries are best carried out at a given age, for the strategy pursued in this paper, a comparison among ninth graders is more appropriate because the **G-8 reform** affected students based on their grade level in a certain school year.

Moreover, national **PISA** extensions (**PISA-E**) were conducted for the years 2000, 2003 and 2006. Each of them consists of about 40,000 students. By oversampling less populated federal states, these extensions allow for more robust comparisons of educational performance between the German federal states.<sup>10</sup> However, **PISA-E** was discontinued in 2009 and replaced by the *federal state comparison test* which is conducted by the Institut zur Qualitätsentwicklung im Bildungswesen (**IQB**). This new *comparison test* aims to assess national educational standards determined by the Standing Conference of the Ministers of Education and Cultural Affairs (**SC**) of all federal states instead of by the **OECD**. Since then, each extension of this *comparison test* covers only a particular domain (reading in 2009, mathematics and science in 2012), which prohibits their use for analyzing the entire period considered in this study (until 2012).

Nevertheless, Andrietti (2016) or Huebener & Marcus (2017) use data from the national **PISA** extensions for the years 2000, 2003, and 2006. They complement them with single waves of **PISA-I**, in the case of Andrietti (2016) only for the year 2009 and in the case of Huebener & Marcus (2017) additionally for 2012.

<sup>9</sup>Some background information on the **OECD's** **PISA** data, its advantages for measuring educational outcomes as well as on the representativeness of these data across states, schools and over time is provided in Appendix III.1.4.

<sup>10</sup>For this purpose, one day after the students for the **PISA-I** samples had taken their test, additional students in each federal state were randomly selected to undergo the same testing procedures for the **PISA-E** test in which they had to answer an additional national questionnaire.

Only *grade-based* PISA-I samples provide all three testing domains consistently for each test year. Therefore, to have consistent comparability across the studies used, this chapter of my doctoral thesis avoids mixing PISA-E and PISA-I datasets.<sup>11</sup>

As my thesis focuses on the academic track (*Gymnasium*), only schools of this type are included in the sample. They make up more than one third of the *grade-based* PISA-I sample which approximately corresponds to the real share of students in *Gymnasium*. Finally, the analysis is restricted to variables derived from the questionnaire answered by students and their parents (the *student-dataset*). Thus, this paper relies on the *grade-based* PISA-I sample to construct a representative repeated cross-section of students in grade nine of the *Gymnasium*. This allows me to analyze the increase in IEOp due to the G-8 reform by using variables based on PISA test scores and the tested students' available background characteristics.

**Descriptive Statistics** Regarding the main outcome variables, the PISA test scores in the domains of reading, mathematics and science are above the German average when focusing on students in the academic track of secondary school. A typical ninth grader in *Gymnasium* achieves results that are about 60 points higher than for the average German ninth grader. This difference corresponds to about an entire *proficiency level*, that is, the value-added of one school year. With respect to the three testing areas, students perform worst in reading literacy. They stagnated or even slightly deteriorated in their reading skills between 2000 and 2012. This observation is in line with reports on German PISA results for the 2000s which show that students perform better in mathematical and scientific than reading tests (e.g. Klieme et al. (2011)). The average scores in mathematics (about 580) exceed those in reading (about 570). Students perform best in science, reaching up to 590 points (see Table III.3 in Appendix III.1.2).

Furthermore, in all three domains the median exceeds mean test scores. This indicates that there appears to be more variation at the lower end of the performance scale, with more students performing relatively badly, thus pushing the mean down. The mean/median comparison and its development may be regarded as first sign for whether IEOp changes over time. The data show that median and mean deviate only slightly more after than before the reform. The same applies to the variance of test scores which does not change significantly over time. Finally, the analysis dataset contains more than 60 schools per test year across all federal states and on average the number of students increases with each test cycle (see Table III.3 in Appendix III.1.2 for an overview).

Moreover, Figure III-4 in Appendix III.1.1 provides a descriptive analysis based on the used *grade-based* PISA-I dataset for different subgroups. For instance, students from academic households achieve slightly higher scores than those from non-academic ones.

<sup>11</sup>In 2000, there was no specific *grade-sample based* PISA-I sample available from the IQB. However, PISA-2000 being the PISA-2000-E dataset is *ninth grade-based* (Baumert et al. 2002). Instead of the usual 80 replication weights, only one weight is provided.

### 3.3.2 Outcome Measure: Inequality of Educational Opportunity (IEOp)

The idea that societies should distribute opportunities equally has a long tradition within political philosophy. Following Rawls (1971) seminal contribution and its discussion (e.g. Sen (1980)), the notion established that a prerequisite for measuring Inequality of Opportunity (IOp) is distinguishing whether a form of inequality is acceptable or not within a society.<sup>12</sup> However, these ideas only started to capture the more widespread attention of economists when scholars such as Roemer (1998) translated these philosophical concepts into a more formal theoretical framework. Since then, an empirical literature has emerged, proposing several methods on how to estimate IOp as shown in recent survey articles by Ramos & Van de gaer (2016) and Roemer & Trannoy (2015).

In the following, I formulate a model regarding how to measure IEOp in line with Ferreira & Gignoux (2011, 2013). To begin with, it is useful to define a set of concepts:

- An *advantage* denotes an individual achievement. Studies typically focus on income; in this chapter, the achievement corresponds to educational outcomes as measured by PISA test scores.
- The vector of *efforts*,  $E$ , denotes the set of variables that influence the outcome variable (*advantage*) and over which the student has control (e.g. choice of time for studying).
- The vector of *circumstances*,  $C$ , denotes the set of individual characteristics which are beyond the student's control, for which one cannot be held responsible, e.g. your family household's socio-economic status (SES), parental education, gender, ethnicity or innate ability/talents.

Consider a sample of  $S$  students indexed by  $i \in \{1, \dots, S\}$ . Each student  $i$  can be described by a set of attributes  $\{y, C_n, E_m\}$ , where  $y$  denotes an *advantage* (here test scores),  $C_n$  is a vector of  $n$  discrete *circumstances* and  $E_m$  denotes the vector of  $m$  discrete *efforts*. Without loss of generality, this model could be extended to the case of having continuous elements in the vectors of *circumstances/efforts*. Thus, we can represent the population by a  $(n \times m)$  matrix  $[Y_{nm}]$  with a typical element (*cell*)

$$y_{nm} = g(C_n, E_m) | C \in \Omega, E \in \Theta, g : \Omega \times \Theta \implies \mathbb{R}$$

being the *advantage* that is a function of both *circumstances* and *efforts*. After selecting the appropriate set of variables capturing *circumstances* characteristics relevant to educational achievement that constitute the  $n$  different vectors  $C_i$  for each student  $i$ , the sample can be split into  $n$  distinct groups of students sharing the same *circumstances* (they are of the same *type*). Similarly, the sample can be split into  $m$  distinct groups of students exerting the same level of *efforts*, but having different *circumstances* (they belong to the same *tranche*). Together *types* and *tranches* form the *cells*.

<sup>12</sup>There is strong experimental evidence that people distinguish acceptable (fair) and unacceptable (unfair) income inequality (Cappelen et al. 2010, Almås et al. 2011). It tends to be acceptable if differences are due to individual responsibilities (*efforts*), but not acceptable if these are due to *luck* (*circumstances*). Lefranc & Trannoy (2017) show how *luck* can be incorporated as an intermediary category between *circumstances* and *efforts*.

In the context of this chapter, when assuming talents to be distributed normally across the whole population, the concept of Inequality of Educational Opportunity (**IEOp**) can be translated as follows. Students who work harder and put in greater *efforts* should be rewarded by achieving good educational results regardless of their specific *circumstances*. Hence, unfair **IEOp** corresponds to differences in educational achievement between students who put in the same *efforts* but only differ in terms of their *circumstances* (*compensation principle*). In contrast, disparities in test results driven by variation in individual *efforts* are acceptable (*reward principle*). Thus, **IEOp** resembles differences between students that can only be attributed to *circumstances* beyond their control.

Deriving a measure of **IEOp** involves two steps: an *Estimation Phase* to transform the original distribution  $[Y_{nm}]$  into a smoothed one  $[\tilde{Y}_{nm}]$  reflecting only the unfair inequality in  $[Y_{nm}]$ , and the *Measurement Phase* which thereon applies a measure of inequality. Following the **IOp** literature, I apply an *ex-ante*, *between-types inequality* measurement approach.<sup>13</sup> As *efforts* are not directly observable, this is also in line with the *indirect* methods to measure **IOp**, because the estimation is based solely on the observed marginal distribution of *advantages* (test scores) given by the vector  $y = \{y_1, \dots, y_S\}$  and on the joint distribution of *advantages* and *circumstances* over the sample population  $\{y, C_n\}$ . Therefore, I follow the measurement approach of Ferreira & Gignoux (2013) which has fewer requirements for data availability than a non-parametric approach. The reason is that the more precisely one tries to design the partition, the smaller *cells* become. Thus, large datasets (best with panel structure) are necessary to conduct a useful non-parametric *within-tranche inequality* decomposition (Checchi & Peragine 2010).

Consequently, this chapter adopts a parametric, *ex-ante* estimation approach to derive **IEOp** measures. I model test scores ( $y$ ) as a function of *circumstances* ( $C$ ) and *efforts* ( $E$ ), as  $y = f(C, E)$ . *Efforts* can also depend on *circumstances*, i.e.  $E = E(C)$  which implies  $y = f(C, E(C))$ . Within this framework, for instance, innate ability is considered to be an unobserved *circumstance* factor that may influence test scores directly through cognitive skills, but also indirectly via its impact on work ethic and other characteristics associated with *efforts*. However, *efforts* cannot vice versa change other relevant *circumstances*, such as gender or parental education.<sup>14</sup> Moreover, as PISA evaluates students in the ninth grade, they are on average about 15 years old. Hufe et al. (2017) argue that choices made before an age of maturity (16) are likely beyond an individual's control. Thus, it is plausible to assume that tested students are (if at all) only partially responsible for their choices, and most unobserved factors would be *circumstances*.

<sup>13</sup>One distinguishes between an *ex-ante* and *ex-post* approach. This refers to how one evaluates **IOp**, thus, to which normative welfare criterion is chosen. Before *effort* is realized (*ex-ante*), following van de Gaer's "mins of means" criterion, **EOp** is achieved equalizing mean outcomes across *types*. **IOp** is measured as *between-types inequality* satisfying *ex-ante* compensation. After *effort* is realized (*ex-post*), following Roemer's "means of min" criterion, **EOp** is achieved eliminating inequality within *tranches* satisfying *ex-post* compensation. Fleurbay & Peragine (2013) show that *ex-post* and *ex-ante* compensation are incompatible. However, if *efforts* are distributed independently from *circumstances*, *ex-post* will be equivalent to *ex-ante* **EOp** (Ramos & Van de gaer 2016, proposition II).

<sup>14</sup>See Appendix III.1.5 for a discussion of how the concept of ability is considered in the context of measuring **IEOp**.

In summary, my model of measuring **IEOp** considers the role of *circumstances*, *efforts* and their interplay. Following [Ferreira & Gignoux \(2013\)](#) a linear functional form is used:

$$y_i = C_i' \beta + E_i' \gamma + e_i \quad (3.1)$$

$$\text{with } E_i = C_i' \delta + u_i \quad (3.2)$$

$C_i$  is a vector capturing *circumstances* variables and  $E_i$  is the unobserved vector of *efforts* per student  $i$ . However, the aim being to estimate the full effect of *circumstances* on scores, i.e. both the direct and indirect effect on scores (via their impact on *efforts*), I estimate the reduced form model:

$$y_i = C_i'(\beta + \gamma\delta) + (e_i + u_i'\gamma) \quad (3.3)$$

$$\text{i.e. : } y_i = C_i' \rho + z_i, \text{ where } \rho = (\beta + \gamma\delta) \text{ and } z_i = (e_i + \gamma u_i) \quad (3.4)$$

The residual,  $z_i$ , includes both unobserved *efforts* and unobserved *circumstances*. But at this point, the aim is to estimate the mean score outcome of each *type* conditional on *circumstances*:

$$\hat{y}_i = C_i' \hat{\rho} \quad (3.5)$$

This will create a new, simulated distribution of scores,  $\hat{y} = \{\hat{y}_1, \dots, \hat{y}_S\}$ . Thus, every student is assigned the value of her opportunity set (which in a linear regression corresponds to the expected score conditional on *circumstances*). This linear model can be estimated using an Ordinary Least Squares (OLS) regression providing the vector of predicted test scores (the *smoothed* distribution).

Having assigned each individual the value of their opportunity set, the second step, the *Measurement Phase*, then involves calculating inequality in this new distribution, using a particular inequality index,  $I(\cdot)$ . To estimate **IEOp**, one would estimate the following ratio:

$$\hat{\theta}_{IEOp} = \frac{I(\hat{y}_i)}{I(y_i)} = \frac{I(C_i' \hat{\rho})}{I(y_i)} \quad (3.6)$$

i.e. the ratio between inequality in *circumstances* (the simulated distribution) and total inequality (actual distribution of scores). Thus, instead of using an absolute measure, I use a relative measure of **IEOp**. This is also suited best to evaluate the reform effect when comparing treatment and control groups over time, because the relative change is of most interest and can be interpreted most intuitively.

Now, the remaining issue is what inequality index  $I(\cdot)$  to use. The literature on **IOp** in income has used the Mean Log Deviation (MLD) index due to its desirable properties (e.g. path independence). However, [Ferreira & Gignoux \(2013\)](#) show that the MLD is not appropriate for measuring inequality in **PISA** data. The reason is that it is not ordinal invariant to the standardization of **PISA** test scores. Instead, the authors prove that the variance is the most appropriate inequality index for measuring **IEOp**. Being an

absolute measure of inequality itself, the variance is ordinally invariant in the test score standardization and satisfies the most important axioms to be qualified as meaningful inequality measure, i.e. (i) symmetry, (ii) continuity and (iii) the transfer principle.

Overall, the variance satisfies the requirements to be an appropriate inequality index for the proposed Inequality of Educational Opportunity (IEOp) measure which then can be calculated as follows:

$$\hat{\theta}_{IEOp} = \frac{\text{variance}(\hat{y})}{\text{variance}(y)} \quad (3.7)$$

This measure is attractive for various reasons. First, it is the coefficient of determination ( $R^2$ ) of an OLS regression of test scores on *circumstances*  $C$  variables which eases measurement procedures.<sup>15</sup>

Second, as shown in Ferreira & Gignoux (2011), the  $R^2$  results in a meaningful summary statistic, the lower bound of the true IEOp. As the subject of concern is the total joint effect of all *circumstances* on educational outcomes as measured by test scores, the object of interest is to understand what percentage of the variation in scores,  $y$ , is causally explained by the overall effect of *circumstances* (directly and indirectly via *efforts*). With *efforts* being treated as generally unobserved omitted *circumstances* variables - if we observed them, they would only lead to a finer partitioning of  $[Y_{nm}^i]$ , which would further increase the IEOp measure. Therefore, the  $R^2$  measure,  $\hat{\theta}_{IEOp}$  in Equation (3.7), is a valid lower bound estimate of the joint effect of all *circumstances* on educational achievement. In other words, it is the lower bound of the share of overall inequality in educational achievement that can be explained by predetermined *circumstances* (a lower-bound estimate of *ex-ante* IEOp).<sup>16</sup>

Third,  $\hat{\theta}_{IEOp}$  is a relative measure of IEOp that is cardinaly invariant to the standardization of test scores. Moreover, one can decompose the IEOp measure into components for each variable in the *circumstances* vector which corresponds to a *Shapely-Shorrocks* decomposition.

### 3.3.3 Control Variables: Measuring Circumstances

Regarding the selection of relevant control variables, this study follows the most common approaches in the literature (e.g. Ferreira & Gignoux (2013)). The control variables represent *circumstances*, factors which a student cannot influence, but which can determine the dependent variable of interest, cognitive skills, as measured by test scores. Moreover, applying new machine learning methods (Brunori et al. 2019), such as regression tree and random forest algorithms, the data confirm that my choice of control variables is appropriate with respect to detecting relevant groups of *circumstances* (see Appendix III.1.5).

<sup>15</sup>The only caveat is that this model cannot estimate the effect of individual *circumstances*. As elements of  $\hat{p}$  may be biased due to omitted variables, one cannot interpret them as causal effect of certain *circumstances* on scores.

<sup>16</sup>Niehues & Peichl (2014) outline how an upper-bound can be estimated in order to find boundaries for IOp estimates. But this method has not yet been widely applied, because of data requirements (e.g. it needs panel data).



Control variables can be divided into, student-level *circumstances*, such as personal characteristics, and socio-economic family background variables, such as parental household characteristics. Table 3.1 provides an overview of the main control variables. Students are on average 15.43 years old. The share of female students in the total tested student population is slightly higher than the one of male students. This reflects the fact that in recent years female participation in *Gymnasium* has been steadily increasing (Prenzel et al. 2013). The variable *migration background* indicates that about 16.8% of students have at least one foreign-born parent. But the variable *language spoken at home* improves the extent to which one controls for the student's migration background. As depending on the level of parental integration, one can expect that not all students with migration traits speak a language other than German at home. Less than half of the number of students with foreign traits indicate that they speak a different language (than German) when talking to family members. I classify all individual characteristics (*gender, age, migration background*) as *circumstances*.

Another set of control variables involves socio-economic family background variables. An important *circumstance* is a student's parental education background which serves as an indicator for potential support opportunities available to the student. To measure parental education, I rely on the *International Standard Classification of Education (ISCED)* index. It serves to identify whether at least one parent has achieved an academic degree, *ISCED* level 5 or 6, in which case they would constitute an *academic household*. Table 3.1 shows that about 60% of students live in such households.

As indicators for the socio-economic status (*SES*) environment in which a student grows up, I use first the *number of books at home*. This variable is generated in all *PISA* studies and has been shown to be a good proxy for the family *SES*, because household income is highly correlated with the amount of books in the household. It is plausible to assume that, at the age of 15, students are still financially dependent on their parents. Moreover, access to culture is mostly influenced by the opportunities offered in the household in which a child grows up. Thus, it is generally accepted that for students of age 15 the *number of books* variable represents *circumstances* that control for family *SES*. I take the range of 101-500 books as a base category for this variable, because approximately 50% of students in the sample live in such a household. Similarly, the *International Socio-Economic Index of Occupational Status (ISEI)* can be taken as a further control variable for socio-economic background. Higher *ISEI* scores correspond to higher levels of parental occupational status on a scale from zero to 90.<sup>17</sup>

As a control for family structure characteristics, I consider whether a student lives in a single parent household which serves as an indicator for whether a student has grown up in a more stressful environment. About 13% of all students are raised under such

<sup>17</sup>The *International Standard Classification of Occupation (ISCO)* can serve as an alternative for describing parental *SES*. The construction of this index involves obtaining parents' occupational data by asking open-ended questions, the responses to which are coded into *ISCO* codes. But this is not available for all *PISA* datasets, in contrast to the mapping of *ISCO* into *ISEI* indices. See Ganzeboom et al. (1992) for further details on this methodology.

*circumstances*. In addition, I also consider *employment status* dummies for both mother and father. By controlling for parental time availability and family structure, aspects that influence the environment in which a student can study are taken into account. In the sample, the vast majority of fathers work full-time (FT), whereas the largest share of all mothers is part-time employed (PT) (about 44%). This is consistent with the predominant family model in Germany during the 2000s consisting of the father as main bread-winner and a part-time working mother mainly in charge of child care.

**Table 3.1:** Descriptive Statistics: Control Variables for *Circumstances*

Time Period (2003-2012)	Mean	SD	Min-Max	Missings (SD)
<b>Individual Characteristics</b>				
Female	0.5289	0.4989	[0-1]	0
Age in years	15.43	0.49	[13,75-17,25]	0
Language spoken at home ( <i>Base: German</i> )	0.0552	0.2285	[0-1]	0.0060 (0.0774)
Migration background ( <i>Base: German</i> )	0.1679	0.3738	[0-1]	0.0060 (0.0774)
<b>Parental Characteristics</b>				
Parental Education:				
(highest <b>ISCED</b> level)				
# ISCED-level (5-6):	0.6285	0.4832	[0-1]	
# ISCED-level (3-4) ( <i>Base cat.</i> ):	0.2812	0.4495	[0-1]	0.0371 (0.1890)
# ISCED-level (1-2):	0.0532	0.2244	[0-1]	
<b>Socio-Economic Status</b>				
Number of books in a household:				
# more than 500:	0.2029	0.4022	[0-1]	
# 101-500 ( <i>Base cat.</i> ):	0.4703	0.4991	[0-1]	0.0497 (0.2174)
# 11-100:	0.2579	0.4375	[0-1]	
# max. 10:	0.0193	0.1375	[0-1]	
Highest- <b>ISEI</b> -level of a job in the family	57.1536	17.2042	[0-90]	0.0177 (0.1317)
<b>Family Characteristics</b>				
Single parent households ( <i>Base cat.: No</i> )	0.1317	0.3382	[0-1]	0.0808 (0.2726)
Father - employment status				
# full-time (FT) ( <i>Base cat.</i> ):	0.8120	0.3907	[0-1]	
# part-time (PT) :	0.0584	0.2345	[0-1]	0.0728 (0.2598)
# unemployed (UE) :	0.0251	0.1564	[0-1]	
# out-of-labor force (OLF) :	0.0318	0.1753	[0-1]	
Mother - employment status				
# full-time (FT) ( <i>Base cat.</i> ):	0.2972	0.4570	[0-1]	
# part-time (PT) :	0.4379	0.4961	[0-1]	0.0603 (0.2381)
# unemployed (UE) :	0.0452	0.2078	[0-1]	
# out-of-labor force (OLF) :	0.1593	0.3660	[0-1]	
Number of students	13,756	<b>G-8 reform</b> dummy: 0.4573 (0.4982)		

*Notes:* This table reports summary statistics for the sample of ninth graders in *Gymnasium* pooling the data for main period studied (**PISA** 2003, 2006, 2009 and 2012) and is weighted by the sampling weights provided in the **PISA** dataset (compare Appendix III.1.4). In the comments column, the amount of missing observations is provided and standard deviations are reported in parentheses. For categorical control variables, the base category is indicated by italics. Finally, the number of observations and the **G-8 reform** dummy share is provided.



### 3.4 Empirical Strategy

Estimation proceeds in two steps. First, appropriate measures of **IEOp** need to be estimated given the available outcome and control variables in the data. Second, the quasi-experimental variation of the **G-8 reform** allows to identify the causal effect of increased **learning intensity** on **IEOp**, by using a Difference-in-Differences (**DiD**) strategy based on forming reasonable treatment and control groups.

#### 3.4.1 Estimating Inequality of Educational Opportunity (IEOp)

In a first step, **IEOp** will be measured using  $\hat{\theta}_{IOP}$ , as defined in [Equation \(3.7\)](#) in [Section 3.3.2](#). This measure requires estimating the coefficient of determination ( $R^2$ ) from an OLS regression of **PISA** test scores on the different *circumstances* variables that are listed in the previous section. Thus, the following regression model is estimated separately, by federal states that form the respective treatment or control groups, and by **PISA** test wave:

$$Y_{ist} = \beta_0 + \beta_1(Individual\ Characteristics)_{ist} + \beta_2(Parental\ Characteristics)_{ist} \\ + \beta_3(Socio-Economic\ Status)_{ist} + \beta_4(Family\ Characteristics)_{ist} \\ + FE(school)_{ist} + \varepsilon_{ist} \quad (3.8)$$

where  $Y_{ist} = \{stdpvread_{ist}; stdpvmath_{ist}; stdpvscie_{ist}\}$  are test scores of student  $i$  in state  $s$  at time  $t$  in one of three **PISA** domains. To ease the interpretation of  $\beta$  coefficients, I standardize scores for the effects to be measured as percentages of an international standard deviation in the **PISA** test.<sup>18</sup>

This baseline regression model needs to be adjusted to take the following two issues into account. First, to allow for the extrapolation of findings to Germany's entire high school student population, the notion of external validity has to be considered ([Meyer 1995](#), [Bertrand et al. 2004](#)). This requires the data sample to be as representative as possible with respect to the student population in the ninth grade of **Gymnasium** in the time period under investigation (mainly 2003 to 2012). Thus, the model is estimated using a Weighted Least Squares (WLS) regression with the population weights provided in the data.<sup>19</sup> Second, the sampling strategy may induce some correlation among observations of the same unit (state/school). Therefore, I adjust regressions by calculating standard errors based on available replication weights in the **PISA** data and allow for clustering at the level of federal states, the level at which the reform has been implemented. Following the **OECD** guidelines, in [Appendix III.1.5](#), I explain how to estimate standard errors for the **PISA** data used in this study.

<sup>18</sup>Appendix [III.1.4](#) provides details on the test metric. Note that in my notation until [Section 3.5.2](#), I focus on the time period (2003-2012) with the general reform time set to take effect between 2006 and 2009, as defined in [Section 3.4.2](#) (see also [Appendix III.1.5](#)). Moreover, the regression model can also be estimated separately by treatment and control groups only twice for the pooled pre-reform ((2000-)2003-2006) and post-reform (2009-2012) samples.

<sup>19</sup>[Baumert & Prenzel \(2008\)](#) discuss the **PISA** sampling strategy and the generation of population weights. They argue that for the **PISA-E** data certain student groups might have been over- or underrepresented, and that provided weights can be used to correct for this. These arguments also apply to the **PISA-I** data.

As explained in [Section 3.3.3](#), the control variables that measure *circumstances* in [Equation \(3.8\)](#) fall into four categories: Individual Characteristics (IC), Parental Characteristics (PC), Socio-Economic Status ([SES](#)) and Family Characteristics (FC) (for an overview, see [Appendix III.1.5](#)). Individual Characteristics include the *circumstances* variables: *age*, *gender* and *migration background*. As students were sampled based on attending the ninth grade, by controlling for *age*, differences in school entrance age (e.g. due to maturity) are taken into account. Controlling for *gender* considers the existence of any subject-specific differences in academic test score performance between male and female students ([Niederle & Vesterlund 2010](#)). *Migration background* has also been shown to be important in explaining the academic achievements of students in Germany ([Klieme et al. 2011](#)). On average, having a migration background is negatively correlated with performance due to its implications on non-cognitive skills, such as self-esteem.

Socio-Economic family background control variables include: Parental Characteristics such as *parental education* levels, socio-economic status ([SES](#)) indicators such as the *number of books in the household*, and Family Characteristics such as *family structure*. A more academically stimulating environment tends to have a positive impact on cognitive skill formation. In that regard, *parental education* can be assumed to constitute *circumstances* that capture investments into a student's early childhood. Similarly, a favorable [SES](#), as measured by higher [ISEI](#) index values and/or more books available in a household, should have a positive impact on a student's test scores. Higher [SES](#) of the family in which a student grows up could be an indicator for better and easier access to support for dealing with school-related work. Otherwise, growing up with a single parent or with unemployed parents might have a negative effect on test scores, because such family conditions are more likely to be associated with adverse factors for skill formation or limited access to out of school support opportunities.

In addition to control variables at the level of student  $i$ , the model in [Equation \(3.8\)](#) includes fixed effects (FEs) at the school level. First, adding school fixed effects allows me to capture quality differences among schools that can also exist within a federal state, and to control for other school-level *circumstances*. Second, applying school fixed effects allows to control for characteristics both on the school and state level, because federal states are in charge of school policy. Moreover, as the [PISA](#) test is not conducted in the same schools over the years, school fixed effects are wave-specific. Thus, they also capture year fixed effects when pooling before and after reform periods.

As a robustness check, a pooled version of [Equation \(3.8\)](#) is conducted, using only fixed effects (FEs) at the state level. Then, state FEs consider time-invariant differences in the outcome variables between federal states due to, for instance, distinct political preferences for school policies neglecting differences between schools. The federal state in which a student attends secondary school represents a *circumstance* variable beyond a student's control, because parents decide on where to reside. Though in theory students may have some influence over which school they attend, their control is likely very

limited at age 10. In fact, estimation results do not change much using either only federal state or only school FEs (which shows concerns on potential sorting at the school level not to be relevant). Consequently, it is sufficient to control only for school FEs in the main estimation specifications.

### 3.4.2 Definition of Treatment and Control Groups

The **G-8 reform** and its implementation at different points in time at the federal state level can be exploited as a quasi-experiment to identify the effect of increased **learning intensity** on a measure of **IEOp**. This requires categorizing the 16 federal states into treatment and control groups for each **PISA** test wave. **Table 3.2** shows how useful treatment and control groups can be formed, based on the implementation of the reform and the timing of this process across federal states.

For seven out of fourteen states in which a reform took place, the introduction of the **G-8 reform** occurs between 2006 and 2009. Therefore, **PISA** 2009 is the first post-treatment wave of ninth graders tested in these states, and regression models including the 2012 wave capture the “medium-term” effect. Therefore, I define the model covering period 2003 to 2012 as the *Model Base*.<sup>20</sup>

**Baseline Model** The reform takes effect in between 2006 and 2009. **Table 3.2** shows that seven federal states can be classified as treatment group, because tested ninth graders were only in the **G-8 model** from 2009 onwards. *Treatment Group T2* includes Baden-Württemberg (BW), Bavaria (BV), Lower Saxony (LS), Bremen (BR), Hamburg (HB), Berlin (BE), and Brandenburg (BB). However, the East German federal states are still likely to be different from the West German states. For instance, many teachers in East Germany were still educated in the former German Democratic Republic (GDR). Hence, for the main results I focus on West Germany only, which means that the main *Treatment Group T* consists of BW, BV, LS, BR and HB. Finally, excluding the city states of HB and BR, the most homogeneous *Treatment Group T1* consists of the three territorial West German states BW, BV and LS. Together with T2, T1 is used for robustness checks.

The control group in the main specification, *Control Group C*, consists of two other territorial states in West Germany: Rhineland-Palatinate (RP) and Schleswig-Holstein (SH). These two states did not move to a **G-8 model** over the considered time period, that is, they always maintained a **G-9 model**. A second control group is made up of the two East German states of Saxony (SN) and Thuringia (TH). These two states had been following a **G-8 model** since 1949, when the former GDR was founded, and chose to maintain their secondary school system after reunification. They form a *hypothetical Control Group Ch* that could be interpreted as the counter-factual of a permanent **G-8 model**. Finally, one can form a *Never-Takers Control Group C-NT* consisting of the four states that never changed the length of *Gymnasium*: RP, SH, SN and TH.

<sup>20</sup>For an overview, see Appendix III.1.5.

**Table 3.2:** “G-8 reform” Treatment/Control Group Allocation of PISA Cohorts per State

Federal State	Reform Enaction	Double Cohort	Treated grade	PISA cohorts affected					(if) Treatment cohort/grade affected							
				2000	2003	2006	2009	2012	2006	2009	2012					
Bavaria (BV)	2004/2005	2010/2011	6	first cohort treated in 6 <sup>th</sup> grade was not in 9th grade in a PISA test year												
	2004/2005	2011/2012	5	C	C	C	T(1)	T(1)	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Lower Saxony (LS)	2004/2005	2010/2012	6	first cohort treated in 6 <sup>th</sup> grade was not in 9th grade in a PISA test year												
	2004/2005	2011/2013	5	C	C	C	T(1)	T(1)	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Baden-Württemberg (BW)	2004/2005	2011/2012	5	C	C	C	T(1)	T(1)	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Hamburg (HB)	2002/2003	2009/2010	5	C	C	C	T	T	-	3 <sup>rd</sup> cohort	6 <sup>th</sup> cohort					
Bremen (BR)	2004/2005	2011/2012	5	C	C	C	T	T	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Berlin (BE)	2006/2007	2011/2012	7	C	C	C	T2	T2	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Brandenburg (BB)	2006/2007	2011/2012	7	C	C	C	T2	T2	-	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort					
Rhineland-Palatinate (RP)	2008/2009	2015/2016	5	C	C	C	C	C	-	-	-					
Schleswig-Holstein (SH)	2008/2009	2015/2016	5	C	C	C	C	C	-	-	-					
North Rhine-Westphalia (NRW)	2005/2006	2012/2013	5	C1	C1	C1	C1	T	-	-	3 <sup>rd</sup> cohort					
Saxony (SN)	since 1949		5	Ch	Ch	Ch	Ch	Ch	hypoth. control-group: always treated							
Thuringia (TH)	since 1949		5	Ch	Ch	Ch	Ch	Ch	hypoth. control-group: always treated							
Saarland (SL)	2001/2002	2009/2010	5	C	C	T	T	T	1 <sup>st</sup> cohort	4 <sup>th</sup> cohort	7 <sup>th</sup> cohort					
Saxony-Anhalt (ST)	2003/2004	2006/2007	9	1 <sup>st</sup> cohort 7 <sup>th</sup> graders												
		2007/2008	8													
		2008/2009	7									-	-	T	-	-
		2009/2010	6									C	C	-	T	T
Mecklenburg-West Pomerania (MWP)	2004/2005	2010/2011	5	1 <sup>st</sup> cohort 8 <sup>th</sup> graders												
		2007/2008	9													
		2008/2009	8									-	-	T	-	-
		2009/2010	7									C	C	-	T	T
Hesse (H) <sup>a</sup>	2004/05 2005/06 2006/07	2010/2011	6	1 <sup>st</sup> cohort 5 <sup>th</sup> graders												
		2011/2012	5									C	C	-	T	T
		2012/2013	5									C	C	C	C	T
		2013/2014	5	C	C	C	C	T	-	1 <sup>st</sup> cohort	3 <sup>rd</sup> cohort					
										-	2 <sup>nd</sup> cohort					

<sup>a</sup> Hesse (H) introduced the reform gradually across three school years (Figure 3-1 and Table III.1), thus is neither treatment nor control group.

*Notes:* In this table, the Treatment/Control Groups are highlighted by rectangular boxes.

For *Model Base* and *Model Robust*:

- Treatment **T** ≡ red box; **T1** ≡ magenta (inner) box and **T2** ≡ red + violet box
- Control Group (**C**) ≡ blue rectangle; **C1** ≡ blue + green rectangle.
- Moreover, TH and S form a hypothetical *Control Group (Ch)* (always **G-8 model**). Ch and C form the never-taker *Control Group (C-NT)*.

Note: An overview of treatment/control groups is given in Appendix III.1.5.

The most comparable setting for the baseline model consists of the *Treatment Group T* and *Control Group C*, because it focuses on West German federal states that are very similar in relevant characteristics. Thereby, this setting still accounts for 40 out of 80.6 million people and thus for 50% of the German population. Hence, it will serve as the main specification for the *Model Base*.<sup>21</sup>

Focusing on a treatment that affects students in grade nine from 2009 onwards, five federal states belong neither to treatment nor control groups. In the first West German state that implemented the reform, Saarland (SL), ninth graders were already in a **G-8 model** by 2006. The same is true for the two East German states of Saxony-Anhalt (ST) and Mecklenburg-West Pomerania (MWP). Moreover, in both states the reform affected students from ninth grade onward, whereas in most other states students were affected from fifth grade onward. In Hesse (H)<sup>22</sup> and North Rhine-Westphalia (NRW), ninth graders were only taught in a **G-8 model** since 2010, after the 2006-2009 window.

**Robustness Model** The *Model Robust* covers the time period 2003 to 2009 and thus considers the effect in response to the reform that is visible in 2009. This effect will be denoted the “short-term” effect of the reform. The treatment groups remain identical to those in the medium-term models (**T/T1/T2**), because only the year 2012 will be dropped in the short-term models with the reform time still set between 2006 and 2009. This also applies to the *Control Group C* consisting of RP and SH and to the *Never-Takers Control Group C-NT* including additionally SN and TH. Now, North Rhine-Westphalia (NRW) as federal state with the largest population in Germany can be added to the Control Group **C**: in the *Model Robust*, ninth graders in NRW were taught in a **G-9 model** over the whole time period (2000)/2003 until 2009. This creates *Control Group C1* consisting of RP, SH, and NRW. The most comparable setting for the robustness models consists of the *Treatment Group T* and *Control Groups C* or **C1**. With the latter group I account for 57.6 out of 80.6 million people, thus for 70% of the German population. Hence, there are two main control groups for the *Model Robust*.

### 3.4.3 Difference-in-Differences Estimation Strategy

The second step of the empirical strategy in this third chapter of my dissertation is a Difference-in-Differences (**DiD**) estimation. The gradual implementation of the **G-8 reform** across federal states allows estimating the reform-induced effect of increased **learning intensity** on **IEOp** by exploiting the differences between comparable treatment and control groups. For example, in the main specification of *Model Base*, there are five states in the treatment group (Baden-Württemberg, Bavaria, Lower Saxony, Bremen, Hamburg) and two states in the control group (Rhineland-Palatinate, Schleswig-Holstein).

<sup>21</sup>However, in **Section 3.5.3**, I also conduct robustness checks using T1, T2 and C-NT (**Figure III-3** in Appendix **III.1.1**).

<sup>22</sup>Hesse (H) is the only federal state that did not implement the reform uniformly for **Gymnasium** at the start of one school year, but successively over three years as shown in **Table 3.2**. Thus, it is not possible to classify Hesse (H) either as treatment or control state in 2009 (without further assumptions) and it has to be excluded from estimations.

Moreover, the pre-reform years cover 2003-2006 (*before*), and 2009-2012 are the post-reform years (*after*). Then, the **DiD** strategy is implemented via the regression model:

$$R_{st}^2 = \delta_0 + \delta_1(Treat_{st} = after_t \times Treat_s) + \gamma_t \times after_t + \xi_s \times Treat_s \\ (+ \alpha X_{st}) + \varepsilon_{st} \quad (3.9)$$

where  $R_{st}^2 = \{R^2(read)_{st}; R^2(maths)_{st}; R^2(science)_{st}\}$  is the estimated coefficient of determination ( $R^2$ ) from Equation (3.8) associated with state  $s$  in test year  $t$  that measures **IEOp** in the three **PISA** domains. *Treat* captures the *Treatment Group*-specific effect and *after* the time trend.  $\delta_1$  is the coefficient of the interaction term, being 1 if a student attends a **Gymnasium** in a treatment state after the implementation of the **G-8 model**: it measures the causal reform effect of increased **learning intensity** on **IEOp**.  $\delta_0$  is a constant (*before* control mean),  $\varepsilon_{st}$  is the regression error term.

$X_{st}$  is a vector of potential state-level variables. It can be used to address concerns about differential implementation effects on the level of federal states imposing the reform (e.g. due to school policies). For robustness checks, I adjust the regression by including school fixed effects capturing any specific effects at the highest level of variation that is not captured by the **DiD** group specific means in Equation (3.9). These fixed effects also incorporate both federal state and year fixed effects, the latter as different schools are randomly sampled for every **PISA** wave. However, when the **DiD** approach is internally valid, results remain robust and the simple **DiD** specification (without  $X_{st}$ ) is sufficient.

### 3.4.4 Selecting Appropriate Treatment/Control Group Settings

**Internal Validity** German federal states share a similar legislative, cultural, economic framework and common qualification standards are coordinated by the **SC**. Thus, exploiting variation in the implementation process of the reform across states is more effective than relying on cross-national variation (Wössmann 2010).

Next, one should consider whether the reform effect is driven not only by the explanatory variable of interest (increased **learning intensity**), but by other non-random factors in response to the reform. One concern with the **DiD** strategy might be that potentially affected students move with their families to a state that has not yet implemented the **G-8 reform**. If such reactions had occurred in a treatment group before the reform had been implemented, the population's composition across treatment and control groups might have changed in a way that would bias estimation results.

However, such anticipatory behavior is very unlikely. First, options for moving between federal states to avoid the **G-8 reform** were limited. The implementation across all federal states was fast: half of all reform states started the transition into shortened duration of the **Gymnasium** within three school years (2003/2004 until 2005/2006). There is no systematic pattern regarding the timing and implementation of the **G-8 reform** and the geographical location of reforming federal states.<sup>23</sup>

<sup>23</sup>The geographical maps in Figure III.2 in Appendix III.1.1 reveal the quick spread of the treatment across states.

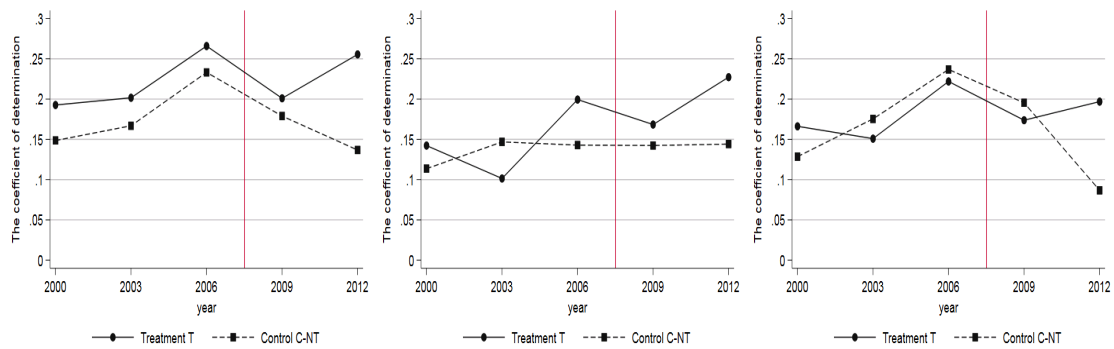


Second, direct and indirect moving costs, including bureaucratic hurdles, have been shown to be reasons why only a few families with children of school-going age move to another federal state in Germany ([Bundeszentrale für politische Bildung 2008](#)). Families tend to move more between municipalities than states.

Third, strategic considerations concerning the competition for access to study programs also support the assumption that bias due to movement between states is unlikely. As a result of the reform, it was obvious that several *double cohorts* would graduate in between 2009 and 2016. This temporary increase in the number of applicants for university studies could inversely affect the probability of students to quickly enter a study program of their choice. Hence, **G-8 model** students could at least insure themselves against the risk of having to take a gap year as their 14<sup>th</sup> year of education. Instead of spending 13 years in school and having to wait one additional year before entering the study program of their choice, having 12 years of schooling before enrolling at a university means that even after one gap year, **G-8 model** students could “save” one year compared to **G-9 model** students completing a gap year

By focusing on a setting in which treatment states implemented the reform in school year 2004/2005, the quasi-experimental design is also unlikely to suffer from estimation bias due to non-random political reasons for introducing the **G-8 reform** slightly earlier or later among federal states. Appendix III.1.5 shows that treatment/control groups are not different regarding the stability of state governments in charge of school policy: political preferences remain stable over the analysis period. Moreover, no systematic change in the transition flows between secondary school tracks is observed due to the **G-8 reform** ([Huebener & Marcus 2017](#)).

Finally, the internal validity of a **DiD** estimation requires the common time trend assumption to hold: without the reform, both treatment and control group would have shown a parallel time trend. This can be confirmed by examining the pre-reform trends in terms of the estimated **IEOp** measure for the main treatment and control groups in [Figure 3-2](#). Moreover, I conduct placebo tests ([Bertrand et al. 2004](#)) as robustness checks in [Section 3.5.3](#) that also confirm the validity of my strategy.

**Figure 3-2: Robustness - DiD Graphs of IEOp measure for Enlarged Treatment/Control Groups**

(a) IEOp measure based on maths      (b) IEOp measure based on reading      (c) IEOp measure based on science

*Notes:* This figure shows the DiD graphs for IEOp measures based on all three test domains. It confirms that the parallel trend assumption holds. Five (*Treatment Group T*) federal states are compared to the never-changing *control group* (C-NT) consisting of four states. Compare also Figure III-6 and Figure III-7 for other specifications showing that trends are not sensitive to alternative compositions of the treatment group and Figure III-5 in Appendix III.1.1. As discussed in Section 3.3, the data used for the main regressions cover the time frame 2003 to 2012.

*Source:* Author's own calculations based on PISA 2000, 2003, 2006, 2009 and 2012.

**Treatment/Control Group Comparison** Due to the quasi-experimental design of the G-8 reform, estimating the effect of the reform on IEOp should not be biased by any selection of students based on pre-reform characteristics. As the identification strategy relies on comparing the change in IEOp for ninth graders attending *Gymnasium* across treatment and control groups before and after the reform, many significant observable pre-reform differences in the control variable sets might weaken the empirical strategy. Following Imbens & Wooldridge (2009), Table III.4 shows standardized means comparison tests for the control variable sets (Table 3.1) concerning all treatment groups and the main control group C. For the baseline model, *Model Base*, the G-8 reform takes effect between 2006 and 2009. Hence, PISA waves 2003 and 2006 constitute the pre-treatment period. Table III.4 shows that treatment and control groups have similar characteristics in terms of the main *circumstances* variables used for the analysis. Moreover, apart from small differences in the level of *circumstances* variables, the pre-reform comparison of groups T and C are robust. This supports the internal validity of the strategy, because the main treatment and control groups consisting of West German states turn out to be comparable. Using smaller or enlarged treatment groups, the pre-reform comparison tests are still robust in combination with the standard control group (C).<sup>24</sup>

In summary, the pre-reform sample means comparison test for the main control variable set (Table III.4) suggests that the DiD estimation approach outlined in Section 3.4.3 is internally valid. This is true at least for *Model Base* when comparing *Treatment Groups T/T1/T2* versus *Control Group C*; and for *Model Robust* when comparing *Treatment Group T* versus *Control Group C1* (see Table III.5).

<sup>24</sup>This supports the internal validity of the estimation strategy: see pre-trend graph in Figure III-7 in Appendix III.1.1.



### 3.5 Results and Discussion

When presenting the results for the outcome variables, **PISA** test scores in each of the three domains, the respective five plausible values are standardized based on the distribution of test scores across the sample of students attending the ninth grade of *Gymnasium* that are taken from the representative *grade-based PISA* data sets (Section 3.3.1).<sup>25</sup> Section 3.5.1 explains the first-step, Section 3.5.2 the second-step results for the baseline model specifications (Section 3.4.2). Section 3.5.3 provides robustness checks with extended treatment and control group settings, while Section 3.5.4 rationalizes the results.

#### 3.5.1 First-Step Result: Inequality of Educational Opportunity Measure

The first step of analyzing the distributional effects of increased **learning intensity** involves deriving the main outcome variable, the measure of **IEOp**, as share in the standardized **PISA** test score variance that can only be attributed to observed *circumstances* (Equation (3.8) in Section 3.4.1). All six sets of control variables that capture *circumstances* are jointly used to derive this **IEOp** measure.<sup>26</sup> Its standard errors are obtained by using replication weights and clustering on the highest level on which the reform was implemented (Bertrand et al. 2004), the federal state level. Finally, population weights take into account the stratified data structure and representativeness of each observation.

When estimating **IEOp**, it is useful to check how *circumstances* variables directly affect cognitive skills as measured by test scores. Detailed regression output per test domain is provided for the main specification *Model Base: T* versus *C* (Table III.6, III.7, III.8).<sup>27</sup> The following patterns can be observed concerning how the *circumstances* variables (as defined in Section 3.3.3) affect test scores. The only control variable changing the direction of its effect on achievement scores depending on the test domain is *gender*. Being female decreases a student's achievement in the **PISA** mathematics test by 45-65% and in the science test by 30-50% in terms of an international standard deviation (SD). The effect size slightly declines in the post-reform period across both treatment and control group. However, female students increase their reading performance by up to 40% in terms of one international SD. This is consistent with the literature on gender-specific achievement differences in educational test outcomes (Niederle & Vesterlund 2010).

<sup>25</sup>For the remainder of this chapter, I restrict the presentation of *first-step estimation* results to test scores that are standardized with respect to the pooled sample of all students in *Gymnasium* that are part of the representative *grade-based PISA* test cohort in any of the test years that form the sample (2003, 2006, 2009, 2012 in *Model Base*) (*stdpvsb3*): This allows me to interpret the coefficients relative to the average student performance over the sample period.

<sup>26</sup>In Section 3.5.3 for robustness check purposes, for all main specifications and each test domain, all results are shown adding step-by-step control variables (covering *circumstances*): from (i) and (ii) constituting control set (I) until (VI) encompassing controls (i), (ii), (iii), (iv), (v), (vi), (vii). See Appendix III.1.5 for details on computing standard errors.

<sup>27</sup>Table III.7 shows the *first-step* results for reading test scores, Table III.6 for mathematics test scores and Table III.8 provides the corresponding output for science test scores. In each table, the columns (1) and (2) refer to *Control Group C*, the columns (3) and (4) to *Treatment Group T*. Within both *Groups*, the first column refers to the “*Before*” reform period (2003-2006), the last one repeats regressions using only “*After*” reform (2009-2012) data.

All the other control variable estimates are fairly robust in their signs independent of the test domain. As expected, the *age* effect is negative. Those, for instance, that started school at an older age or that had to repeat a grade before entering the ninth grade will be older compared to their peers due to factors correlated with below-average performance in test scores. Similarly, having a *migration background* is associated with performing lower in all three testing domains. Additionally controlling for whether a *foreign language is spoken at home*, the negative effect shrinks as expected. Thus, the degree to which a migrant student experiences integration to the host country's standards on a daily family life level, seems to be key for test scores, in particular for the domain of reading.

Regarding the socio-economic status (**SES**) of the household in which a student grows up, a higher *amount of books* than the base category (101-500) is positively correlated with test scores. Likewise, the higher the **ISEI** index of a parental job in the family, the higher is the positive effect on scores.<sup>28</sup> Thus, the **SES** control variables tend to match the literature suggesting that higher family **SES** correlates with beneficial conditions for early childhood development. *Parental education* is also indicative for academic support opportunities, and indeed a positive impact on test scores for both mathematics and science can be found for the variable indicating that a student grew up in an *academic household* (at least one parent with **ISCED** level 5-6). The effect is less important for reading. As mathematics and science are subjects likely requiring more specific and targeted knowledge from parents for them to be able to support their children, this may explain the difference.<sup>29</sup> But Parental Characteristics have less effect on scores once individual *circumstances* are taken into account. Finally, *family structure* and *employment status* show no clear patterns.

In summary, *first-step* regressions demonstrate that for the medium-term horizon most of the *circumstances* variables affect the **PISA** test scores into the expected directions. The fact that these patterns are consistent over varying time horizons and across **PISA** data sets confirms that the chosen *circumstances* variables were appropriately selected (compare also Appendix III.1.5). Furthermore, the explanatory power of these *first-step* regressions remains in a range of about 15-35% across the different specifications. Thereby, **IEOp** measures tend to be higher when measured with respect to mathematical and scientific skills (20-35%) than with respect to reading literary (15-25%). Consequently, the level of the **IEOp** measure found in this chapter can be categorized as a lower bound within the range of the few available **IEOp** estimates for European countries. For instance, [Ferreira & Gignoux \(2013\)](#) find that about 35% of test score variation in **PISA-I** 2006 can be attributed to *circumstances* for the case of Germany, and [Carneiro \(2008\)](#) finds that **IEOp** amounts to about 40% for the case of Portugal.

<sup>28</sup>With the average family's highest job **ISEI** index being 58, an effect on test scores of 0.001 translates into 5.8% of a **PISA** international test standard deviation. See also [Section 3.3.3](#) and [Appendix III.1.5](#) for further explanations.

<sup>29</sup>Furthermore, highly educated parents might be more aware of the greater importance of numeracy skills for labor market outcomes. However, the effects of growing up in an *academic household* are rather insignificantly positive, whereas those of growing up in less educated families are rather significantly negative for test scores.

### 3.5.2 Main Results: The Effect of Increased Learning Intensity on IEOp

In this section, I switch to the *second-step* of the estimation approach, the **DiD** framework. The **IEOp** measure that I just derived by the *first-step* regressions is the share of total variance in test scores which is accounted for by the student's predetermined *circumstances* variables.

**Baseline Model Results** Starting with the main treatment and control group specification, the *Model Base* results are shown in **Table 3.3**. The top panel outlines the **DiD** estimates for reading, the middle panel for mathematics and the bottom panel for science test scores. **IEOp** is calculated with school fixed effects.

**Table 3.3:** Main Results for T vs. C

Subject	IEOp measured as $R^2$			IEOp measured as $R^2$ adj.		
Reading	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.242 (0.057)	0.180 (0.031)	-0.062 (0.065)	0.172 (0.062)	0.154 (0.032)	-0.018 (0.070)
After	0.162 (0.034)	0.213 (0.020)	0.051 (0.039)	0.114 (0.036)	0.192 (0.021)	0.078 (0.041)
Change in $R^2$	-0.080 (0.066)	0.033 (0.037)	<b>0.113</b> (0.076)	-0.058 (0.072)	0.037 (0.038)	<b>0.096</b> (0.081)
Mathematics	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.353 (0.060)	0.267 (0.033)	-0.086 (0.068)	0.294 (0.065)	0.245 (0.034)	-0.049 (0.073)
After	0.190 (0.040)	0.249 (0.027)	0.060 (0.048)	0.143 (0.043)	0.229 (0.027)	0.086 (0.051)
Change in $R^2$	-0.164 (0.072)	-0.018 (0.042)	<b>0.146</b> (0.083)	-0.151 (0.078)	-0.015 (0.043)	<b>0.136</b> (0.089)
Science	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.363 (0.052)	0.215 (0.025)	-0.148 (0.058)	0.304 (0.057)	0.190 (0.026)	-0.114 (0.063)
After	0.173 (0.048)	0.210 (0.023)	0.037 (0.053)	0.125 (0.051)	0.188 (0.023)	0.063 (0.056)
Change in $R^2$	-0.190 (0.071)	-0.005 (0.034)	<b>0.185</b> (0.079)	-0.179 (0.077)	-0.002 (0.035)	<b>0.177</b> (0.084)

*Notes:* Table entries are  $R^2$  measures of **IEOp** (Equation (3.7)). Robust standard errors are in parentheses and were calculated using replication weights following the method as explained in Appendix III.1.5, clustering at the federal state level. **DiD** results are estimated according to Equation (3.9) taking into account population weights. Positive changes in  $R^2$  indicate increasing **IEOp** or decreasing **EEOp** and vice versa for negative changes.

*Background variables used to derive  $R^2$ :*

- (i) Individual Characteristics (IC) I: *age and gender*
- (ii) Individual Characteristics (IC) II: *language spoken at home; migration background* (based on par. birth place)
- (iii) Parental Characteristics (PC): *highest parents' qualification* (*ISCED 1-2/ISCED 3-4/ISCED 5-6*)
- (iv) Socio-economic Status (SES) I: *no. of books in household* (max. 11, 11-100, 101-500, more than 500)
- (v) Socio-economic Status (SES) II: *highest ISEI-level-index [0-90] of job in the family*
- (vi) Family Characteristics (FC) I: *family structure - growing up in single parent household?*
- (vii) Family Characteristics (FC) II: *mother/father working part-time (PT) - mother/father unemployed (UE) - mother/father out of labor force (OLF)*

*Compare:* The first-step regressions of the setting: treatment group **T** vs. control group **C** are provided in **Table III.7**, **Table III.6** and **Table III.8** in Appendix III.1.2.

*Source:* Author's calculations based on PISA 2003, 2006, 2009, 2012 (compare Section 3.3.1).

The DiD table illustrates that the change in IEOp as measured by the  $R^2$  in the *first-step* estimation exhibits a common pattern across all three test domains - IEOp has increased due to the G-8 reform. That is, the share of inequality in test scores that can be attributed to *circumstances* has risen. With the estimate being a lower bound of the true IEOp, the results can be interpreted as follows. At least about 10% of the variation in reading test scores can be additionally attributed to *circumstances* beyond the control of a ninth grade student. For mathematics, at least about 14% and for science at least about 18% of the test score variation can be additionally considered to constitute IEOp. These results are statistically significant, with standard errors computed as explained in Appendix III.1.5. Thus, given initial values of 20-30% in IEOp, DiD estimates would correspond to a relative increase in IEOp of at least 25% in response to the rise in *learning intensity* induced by the G-8 reform. Hence, the increase in IEOp is economically significant. The effects are stronger when IEOp is measured with respect to science or mathematics than with respect to reading test scores.

Zooming in, one can further note that IEOp seems to have considerably decreased in the time period after the reform for *Control Group C*. Instead for the *Treatment Group T*, the level of IEOp appears to have remained practically unchanged across all three domains. In this setting, the increase in *learning intensity* appears to have maintained the role of *circumstances* constant in treated states, whereas without shorter school duration IEOp tends to have decreased. The *Model Base* takes a medium-term perspective as not only the first affected cohorts are taken into account, but data up to 2012 are considered, when the reform had already been fully enacted. By 2012, in most federal states the *double cohort* had already graduated or was about to graduate (Figure 3-1).

**Robustness Model Results** To learn about the robustness of the effects, it is useful to see how results change for the main treatment and control group specification when conducting the same two-step estimation procedure for the *Model Robust* covering only years 2003 until 2009. Therefore, the left-hand panels in Table III.9 in Appendix III.1.2 show the short-term effects of increased *learning intensity* on IEOp focusing mainly on the first student cohorts treated by the G-8 reform for *Treatment Group T* versus *Control Group C*. The DiD estimates remain positive across all test domains. However, the increase in IEOp only reaches levels that rest within a range of about 5-10% of the variance in educational test scores that can be additionally attributed to *circumstances*. However, results are no longer statistically significant at the 5% level. Thus, the relative deterioration in IEOp is lower in the short term - if different from zero at all - compared to its significant size in the medium term (Table 3.3). Otherwise, the underlying patterns of the reform effect also remain robust in the short term. Educational acceleration tends to inhibit students in the treatment group from experiencing any improvements in IEOp. Instead, ninth graders in the control group experience less IEOp as *circumstances* lose explanatory power for academic achievement.

To understand how the **G-8 reform** changed educational opportunities in *Gymnasium*, it is useful to expand the robustness model to consider treatment and control group specifications that bear even more external validity for the German school system. With **C1** about 70% of the German high school student population can be considered in the short-term reform analysis. **DiD** results for this extended treatment and control group specification are shown in the right panel of **Table III.9**. There appears to be no effect on **IEOp** across all three test domains in response to the **G-8 reform**. However, the **IEOp** measures still range between 15 to 25%, their magnitude increasing from reading to mathematics to science. Students in both treatment and control group experience a similar rise in **IEOp**, such that in total the **DiD** effect is canceled out. The **DiD** estimation findings on the effect of the **G-8 reform** are similar across **T/C** and **T/C1** specifications: there is no statistically significant short-term effect of the reform-induced increase in **learning intensity** on **IEOp**.

In summary, the impact of the reform on **IEOp** is robust for the alternative specification. Focusing on *Model Robust* (2003-2009), increased **learning intensity** does not affect **IEOp**, that is, unfair inequality in terms of how much in the cognitive test score variation can be explained by *circumstances* beyond a student's control (right panel in **Table III.9** in Appendix **III.1.2**). Narrowing the control group to include only federal states that did not plan to shorten the duration of their **G-9 model Gymnasium**, a considerable increase in **IEOp** of about 5-10% in terms of additional explanatory power is observable also in *Model Robust* setting, but results are barely statistically significant (left panel in **Table III.9**). However, taking a medium-term perspective on the **G-8 reform** (*Model Base* (2003-2012)) shows that the reform-induced increase in **learning intensity** causally increases the **IEOp** measures (**Table 3.3**). The observed rise in inequality of opportunity is statistically significant and covers at least 25% of the general **IEOp** measure estimated for students attending German secondary schools. Results reveal that for students in *Gymnasium* the lower bound levels of **IEOp** correspond to about 17-35% of the variance in educational outcomes that can be attributed to the role of *circumstances* only.<sup>30</sup> Thus, the main results show that increased **learning intensity** aggravates **IEOp**. The effects are stronger when measured for mathematics and science than for reading.

### 3.5.3 Robustness Checks

**Placebo Test** To evaluate the plausibility of the quasi-experimental identification strategy that allows a causal interpretation of the effects of the **G-8 reform**-induced increase in **learning intensity** on **IEOp**, it is important to conduct placebo tests (Bertrand et al. 2004). Setting the reform to artificially take effect between 2003 and 2006, no statistically significant effects can be detected for any of the main treatment and control group

<sup>30</sup>To investigate whether this increase in **IEOp** is long-lasting, one would ideally need to consider longer time periods, that are not yet available. However, once shifting attention to student cohorts that are far away from the first treated ones, potential new curricular reforms undertaken in response to the initial **G-8 reform** (**Table III.1**) should be taken into account. Instead, it is plausible to assume that medium-term effects on **IEOp** as defined in this chapter are long-lasting given the literature on the persistence of education on lifetime outcomes (Deming 2009).



specifications (**T** vs. **C** in Table III.10 in Appendix III.1.2). In addition to the pre-reform comparison test (Section 3.4.4), this finding supports the internal validity of the estimation strategy, in particular that the common time trend assumption holds. This can also be seen from examining the pre-reform trends in terms of the estimated **IEOp** measure for the main treatment and control groups in Figure 3-2 in Section 3.4.4. Thus, placebo tests confirm the plausibility for interpreting the main estimation results as causal effects of the reform on **IEOp**.

Moreover, multi-level regressions confirm that school level *circumstances* are indeed already considered by school fixed effects. Moreover, using school fixed effects or only federal state effects to measure **IEOp** does not change **DiD** results (Table III.11 in Appendix III.1.2). This indicates that sorting based on schools is not a concern, which also supports the internal validity of the empirical strategy taken.

To further investigate the robustness of my main results, I focus on three margins of interest. First, I analyze how findings change depending on which of the available six control variable sets are included in the *first-step* regression for deriving the **IEOp** measure. Second, I focus on how **DiD** results change when extending or reducing the treatment group. Third, I show how results change for enlarged control groups consisting of states that never changed their academic track.<sup>31</sup>

**Varying the Control Set of Circumstances Variables** To understand how robust **DiD** results remain when changing the amount of control variables chosen to cover predetermined *circumstances*, I analyze how in particular *adjusted R<sup>2</sup>* measures of **IEOp** behave. The *adjusted R<sup>2</sup>* can help to detect which *Control set*<sup>32</sup> combination appears to have most explanatory power among the available *circumstances* variables (Table 3.1). Looking across the **DiD** result tables, including as *circumstances* variables Individual Characteristics (IC), Parental Characteristics (PC) and Socio-Economic Status (**SES**) may be optimal among the six control variable sets. However, the analysis across different sets reveals that for each test domain the final reform estimate of increased **learning intensity** on **IEOp** does not change much across *Control sets* 3 to 6 (see Table III.12). This also provides support for the empirical strategy taken to derive the main results: using all six variable sets in the *first-step* regression. In fact, this approach renders estimates that correspond to the highest *adjusted R<sup>2</sup>* generating *Control set* combination. Moreover, regression patterns stay robust in size and direction independent of which set is used to derive **IEOp**. This is evidence for the quasi-experimental design assumption that assignment to treatment occurred without selection on observables, but randomly.

<sup>31</sup>The main output tables for robustness checks are shown in Appendix III.1.2: Table III.12 to III.13. All of these tables are structured in the same way to provide an overview of **DiD** estimation results of increased **learning intensity** as induced by the **G-8 reform** on **IEOp**.

<sup>32</sup>*Control set 1* provides results based on deriving the **IEOp** measure including only Individual Characteristics (IC) as control variables (that is (i) and (ii) in Section III.1.5). Then, subsequently additional control variables are added, until in set 6 all available *circumstances* are applied together in the *first-step* regression.

**Extending Treatment Groups** Next, it is useful to repeat the estimations with extended treatment groups to investigate the potential external validity of the main results. Therefore, all main regressions (Section 3.5.2) are rerun with *Treatment Group T1* excluding the two West German city states Hamburg and Bremen, and for *Treatment Group T2*, which is **T** plus Berlin and Brandenburg. When the treatment group gets larger, the DiD reform effects become smaller, for instance, in the regression settings with *Control Group C* (Table III.12) in *Model Base*, the increasing effect on **IEOp** declines as we move from **T** to **T2** consistently within each test domain and across all *Control sets*. In summary, despite their increasingly heterogeneous composition, the main results in terms of direction and size are reconfirmed. This supports the potential external validity of the results based on the carefully chosen *T/C Group* specification in the previous section. Thus, focusing on the *Treatment Group T* does not mean that results do not carry implications which are likely to be valid for the entire German secondary school system.

**Extending Control Groups** As mentioned in Section 3.4.2, one could also compare treatment groups with federal states that always maintained the same length for the *Gymnasium*. When using *Never-Taker Control Group C-NT*, the DiD results in all specifications show a smaller increase in **IEOp**. The results for this specification can be seen in Table III.13 in Appendix III.1.2. This may be due to the fact, that if one takes the complementary part of **C-NT**, the hypothetical control group consisting of Saxony and Thuringia, the effects are rather slightly negative, but not significant.

### 3.5.4 Discussion and Interpretation of Results - Potential Mechanisms

To begin with, the key concept of **IEOp** in this chapter is closely related to the issue of social mobility. Estimating  $\hat{\theta}_{IEOp}$  can be regarded as isomorphic to measuring intergenerational persistence of **IEOp**. For the latter, following Galton, one usually regresses a child's ( $y_{it}$ ) on parental outcomes ( $y_{i,t-1}$ ):

$$y_{it} = \beta y_{i,t-1} + \varepsilon_{it}, \quad (3.10)$$

with  $\beta$  as measure of persistence. If one used family background variables instead of parental outcome variables for ( $y_{i,t-1}$ ), then the  $R^2$  measure of immobility (Equation (3.10)) would be similar to  $\hat{\theta}_{IOP}$  (Equation (3.7) in Section 3.3.2) as long as the *circumstances* vector contains mostly family background variables. In this regard,  $\hat{\theta}_{IEOp}$  can be connected to measures of intergenerational educational immobility, which can be used to measure social (im)mobility (such as  $\beta$  Equation (3.10)). In analogy, this is also related to the findings that childhood wealth can serve as a proxy for *circumstances* explaining future wealth inequality (Boserup et al. 2018). Moreover, intergenerational income elasticity and the Gini coefficient of income have been shown to be highly correlated (*Great Gatsby Curve*) which points to a link between **IEOp** and intergenerational social mobility (Black & Devereux 2011). The connection between both concepts can be characterized by two adjoint forces, *upward* and *downward* social mobility.

A decrease in **IEOp** would be indicative for improved *upward* mobility, as it means that *circumstances*, such as the **SES** of the family in which one grows up, became less important for a student's academic performance. Therefore, if lower **IEOp** translates into providing more equalizing learning conditions such that ability, but in particular *efforts* are rewarded, extending **EEOp** would be welfare enhancing in a society with meritocratic preferences. While decreasing **IEOp** may lead to social *upward* mobility for high-performing students from disadvantaged backgrounds, it may also lead to social *downward* mobility for students with beneficial *circumstances* who lack talent and/or *efforts* to maintain their position as soon as *circumstances* were less important for a student's educational outcome.

**Table 3.4:** Score-DiD with Interaction Terms

Subject	(i) Basic	(ii) <b>SES</b> Median	(iii) <b>SES</b> Quartiles	(iv) Academic
<b>Reading</b>				
Treated*post	-0.174 (0.290)	-0.214 (0.291)	-0.137 (0.319)	-0.198 (0.288)
Treated*post* <b>SES</b>	-	0.072*** (0.0274)	0.157*** (0.0389)	-
Treated*post*academic	-	-	-	0.0335* (0.0195)
$R^2$	0.130	0.131	0.148	0.132
<b>Mathematics</b>				
Treated*post	-0.300 (0.262)	-0.382+ (0.264)	-0.322 (0.398)	-0.393+ (0.261)
Treated*post* <b>SES</b>	-	0.148*** (0.0315)	0.284*** (0.0386)	-
Treated*post*academic	-	-	-	0.127*** (0.0218)
$R^2$	0.167	0.172	0.199	0.173
<b>Science</b>				
Treated*post	-0.309 (0.230)	-0.384* (0.231)	-0.382* (0.220)	-0.402* (0.228)
Treated*post* <b>SES</b>	-	0.135*** (0.0306)	0.261*** (0.0414)	-
Treated*post*academic	-	-	-	0.127*** (0.0214)
$R^2$	0.138	0.142	0.162	0.143
Observations	6,649	6,630	3,208	6,483

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. Table entries show the results of a **DiD** on all three test domain scores. Columns (ii-iv) show the results of a **DiD**, as well as its interaction with background variables. Column (ii) shows a distinction between high and low **SES** using the median of the highest **ISEI** in the family, whereas column (iii) displays results where the first quartile according to **ISEI** is assigned low **SES** and the fourth quartile is assigned high **SES**. Column (iv) displays interaction results for academic, a dummy variable taking value 1 if mother or father achieved a university degree and 0 else. All regressions have been conducted with *school* fixed effects and using control set 6 = [(i) + (ii) + (iii) + (iv) + (v) + (vi) + (vii)].  
*Source:* Author's calculations based on **PISA** 2003, 2006, 2009 and 2012.



Table 3.4 presents the results for a DiD based on standardized scores for the main setting 2003-2012. Compliant with existing literature (Huebener et al. 2017) I find no or only a slightly positive effect of the reform on scores. Again the effects for mathematics and science are stronger than for reading. The DiD with SES interaction terms delivers highly significant positive coefficients which are indicative of students from a higher social background having improved more, hinting to an increase in IEOp. Intuitively the more extreme distinction between low and high socio-economic status based on the first compared to the fourth quartile instead of using the median as cutoff leads to stronger results (column (ii) vs. (iii) in Table 3.4). It is worth noting that socio-economic status seems to be more important than growing up in an academic household. As with the DiD on IEOp (measured by  $R^2$  or *adjusted R*<sup>2</sup>), results are weakest for reading, stronger for science and most striking for mathematics. Consequently, these results confirm the main findings of Section 3.5.2.

Table 3.5 depicts the same DiD regression as above but now using tuition as the dependent variable. The graphs presented in Figure III.8 in Appendix III.1.1 justify the assumption of common pre-trends for private tuition, thus giving leverage to this regression. Whereas the basic DiD delivers a slightly negative though insignificant effect of the reform on private tuition, the interaction-term coefficients all carry a positive sign. Admittedly, only the interaction with SES based on highest and lowest quartile is significant at the ten p-value percentage level. Nonetheless all of them point into the same direction: families of higher socio-economic background react to the reform by actively providing their children with more extra tuition. This is further suggestive evidence that disparities in private tuition between different social backgrounds are among the main drivers of the rise in IEOp induced by increasing learning intensity due to the G-8 reform.

**Table 3.5:** Tuition DiD-Results

	(i) Basic	(ii) SES Median	(iii) SES Quartiles	(iv) Academic
Treated*post	-0.103 (0.121)	-0.113 (0.122)	-0.0715 (0.192)	-0.114 (0.122)
Treated*post*SES	-	0.0172 (0.0175)	0.0454* (0.0261)	-
Treated*post*academic	-	-	-	0.0149 (0.0182)
Observations	5,852	5,843	2,821	5,781
R-squared	0.269	0.269	0.297	0.272

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. Table entries show the results of a DiD on private tuition. Columns (ii-iv) show the results of a DiD and its interaction with background variables. Column (ii) shows a distinction between high and low SES using the median of the highest ISEI in the family, whereas column (iii) displays results where the first quartile according to ISEI is assigned low SES and the fourth high SES. Column (iv) displays interaction results for academic, a dummy variable taking value 1 if mother or father achieved a university degree and 0 else. All regressions have been conducted with *school* fixed effects and using control set 6 = [(i) + (ii) + (iii) + (iv) + (v) + (vi) + (vii)].

*Source:* Author's calculations based on PISA 2003, 2006, 2009 and 2012.

Returning to the **G-8 reform**, one can provide the following explanation for the observed findings. First, the fact that increased **learning intensity** had only a limited impact on **IEOp** in the short run may be indicative for the reform heterogeneously promoting both *downward* mobility among students with advantageous *circumstances* and *upward* mobility among those with disadvantaged *circumstances* who having managed to enter the **Gymnasium** may have already undergone a harder selection process.<sup>33</sup>

As the implementation process of the reform suggests, the reform-induced increase in **learning intensity** affected students and their parents by surprise in a manner that they could not adapt to immediately. For instance, being the first one confronted with the newly intensified system, it is harder to adapt as one cannot easily rely on the experiences of older students as was the case for later cohorts in the new **G-8 model**. This may explain why **IEOp** increased only moderately or not at all in the short term. Thus, in the initial reform period, the lag with which favorable *circumstances* adapt to help a student implies that *downward* rather than *upward* mobility forces may have been more relevant for the first affected student cohorts.

Second, in the medium term, after favorable *circumstances* had time to adapt and provide support to the associated students, both *upward* and *downward* mobility would be lessened. For instance, parents are more likely to be aware and prepared to deal with the increased requirements of a **G-8 model** and new forms of additional professional tuition services may become available in response to the reform, based on the experiences of the first affected cohorts. Consequently, favorable *circumstances* may then allow students quicker, easier and better access to a support system helping them to deal with the higher **learning intensity**. Then, increased **IEOp** associated with lower *upward* rather than higher *downward* mobility may be expected in the medium term after the **G-8 reform** was enacted. Descriptive evidence on the evolution of additional, paid tuition for students attending a **Gymnasium** available from **PISA** questionnaires supports the explanation given above (cf. **Figure III-8** in Appendix **III.1.1**). There has been a rise in extra tuition following the reform, with this effect being stronger in the treatment compared to the control group. Moreover, the increase in extra tuition has been more pronounced for students from more privileged family environments (*circumstances*), such as those living in academic households (**Table 3.4**). This trend is confirmed by **Klemm & Hollenbach-Biele (2016)** who analyzed the evolution of private tuition in the same time period using representative survey data for Germany.

Moreover, looking across the medium-term effect evidence (**Table 3.3**), **DiD** estimates of the effect of increased **learning intensity** on **IEOp** reveal some subject-related patterns. The level of **IEOp** is consistently higher for both mathematics and science compared to reading across all treatment and control group specifications. This observation can be interpreted as evidence in favor of the existence of heterogeneous subject-dependent curricular flexibilities.

<sup>33</sup>The high correlation of parental education and a student's probability of entering the **Gymnasium** has been shown (e.g. **Klieme et al. (2011)**) to be persistent in the German school system at least over the last two decades.

In fact, reading skills comprise more general competencies that are not only learnt in language-related courses at school, but also indirectly in other school courses as well as in everyday life - reading being often a necessary prerequisite to simply comprehend, learn or interact with other people. Consequently, variations in *learning intensity* might have less influence on reading skills. In contrast, mathematics and science can be regarded as requiring more specific skills which are mainly accumulated through taught courses at school and less likely to be learnt indirectly through other courses at school or in everyday life. Thus, for the complementary skill set required by mathematics science, it seems to be plausible that positive *circumstances*, such as growing up in an academic household, are relatively more important than for reading. In that context, the fact that the impact of the reform with respect to reading skills is less pronounced, could be interesting for another reason. On the one hand, it might raise the question of whether in order to improve reading skills, current curricula and teaching methods need to be adjusted. On the other hand, it could also only indicate that the reading practice from additional teaching only balances out the negative impact of increased intensity on the actual learning process - which would be another potential part of the explanation for why *IEOp* levels for the domain of reading may be less pronounced than in the other test domains.

However, given the broad definition of *learning intensity* this may still be compatible with findings that the *G-8 reform* itself had small positive effects on mathematics and science test scores in contrast to reading test scores (Camarero Garcia 2012, Andrietti 2016, Huebener et al. 2017, Büttner & Thomsen 2015). Furthermore, Dahmann (2017) shows that cognitive skills measured by IQ proxies did not causally change due to the reform, but only gender-specific differences were reinforced. The fact, that there appear to be no *SES*-specific differences in IQs supports my findings: the observed overall increase in *IEOp* seems to be mainly driven by heterogeneous parental support opportunities to deal with the higher *learning intensity* and cannot be simply explained by potential differences in ability. Finally, as the reform did not adjust teaching-related quality factors for the first affected cohorts, the findings might be regarded to be merely a lower bound for the effects of increased *learning intensity* on performance, in particular as the variance of test scores did not change much.

In summary, even though it is beyond the scope of this third chapter of my dissertation to precisely detect all underlying mechanisms explaining how *IEOp* may be changed and all implications for its translation into both *upward* and *downward* mobility, this chapter reveals one mechanism of how *IEOp* can be causally changed through an educational reform, that is, by increasing *learning intensity*.

### 3.6 Conclusion

The goal of this third chapter of my doctoral thesis has been to shed light onto how Inequality of Educational Opportunity (IEOp) may be shaped by the recent trend of accelerating and intensifying the educational process. This is important to understand the role of *learning intensity* as one policy channel influencing educational opportunities and thus social mobility. Beyond that, the understanding of how institutions affect IEOp is still limited (Ramos & Van de gaer 2016). To approach an answer to these questions, I focus on the academic track of the German secondary school system, the *Gymnasium*. Hereby, I exploit the shortening of school duration from nine to eight years as a quasi-experiment that exogenously increased *learning intensity*. This paper is among the first to combine an evaluation of the G-8 reform with PISA data, that are comparable across federal states and over time, to analyze how increased *learning intensity* causally affects IEOp in Germany. Therefore, I contribute to the still limited literature on measuring IOp with respect to educational outcomes by adding new evidence.

The first step of the analysis involves measuring IEOp as share in the variance of standardized PISA test scores that can be only attributed to *circumstances* beyond an individual's control. Interestingly, the estimated IEOp measures correspond to the levels of estimates for inequality of opportunity in income, pointing to the link between IEOp and (intergenerational) social immobility. The innovative approach of employing a machine learning algorithm to evaluate which *circumstances* variables are relevant can provide us with a second layer of data-driven evidence for the credibility of my IEOp measure (Appendix III.1.5). As a second step, I conduct a DiD estimation strategy to derive causal estimates, with treatment and control groups chosen according to the implementation of the G-8 reform across federal states. The results reveal that the reform-induced increase in *learning intensity* did not affect IEOp in the short term. Instead, in the medium term IEOp significantly increases for affected student cohorts. These findings can be rationalized by differential compensation possibilities for higher *learning intensity* depending on parental resources in terms of the capacity to pay for additional tuition, which may also explain the increased use of private tutoring as documented by Hille et al. (2016). This interpretation is also supported by the outcomes of a DiD estimation with interaction terms on PISA-scores which allows distinguishing the effects by the students' socio-economic background (Table 3.4 and Table 3.5). Moreover, results point to the existence of subject-dependent curricular flexibilities, with mathematics/science being more inflexible, that is, more responsive to changes in curricular intensity than reading.

This chapter also contributes to the literature on evaluating this German school reform which is still controversially debated. It shifts attention in the evaluation of the G-8 reform onto distributional concerns. I show that the G-8 reform can be considered to be a *selective* reform that at least maintains test results, but at the same time increases IEOp, and not to be an *inclusive* reform that at least maintains test results while reducing IEOp (Checchi & van de Werfhorst 2018).

To lower **IEOp** despite higher **learning intensity**, whole-day schooling and methods reducing the dependence of educational support on *circumstances* may be a solution (Deckers et al. 2019). Alternatively, to maintain equality of opportunity, when reducing school duration without adjusting the support schemes at school, the curriculum may need to be reduced accordingly.

Beyond the narrow context of the **G-8 reform**, there are two broader issues this chapter touches on. First, the interaction of **IEOp** and social mobility is likely to be very important for understanding phenomena such as the high persistence in the observed intergenerational transmission of educational achievement. Generally, it would be interesting to evaluate social mobility in regard of *upward* and *downward* mobility. This component seems to be still neglected, in the sense that the focus appears to have shifted onto improving *upward* mobility, while ignoring that this cannot be discussed independently from removing rigidities that potentially limit *downward* mobility. Thus, understanding the effects of compressing education on **IEOp**, and its implications for social mobility are highly relevant.<sup>34</sup> Second, the factor of time compression in the context of education appears to have been largely neglected so far and more research on this topic is needed. Politicians consider changes on the margin of educational intensity, but as the **G-8 reform** shows, this may involve unintended and underestimated welfare costs. A better understanding of the relationship between schooling duration, intensity, and **IEOp** would also be important in the context of evaluating the conditions of welfare benefits and cost of investments into the educational system. As the costs associated with the misallocation of talents due to a lack of social (educational) mobility may be considerable (Philippis & Rossi 2019, Boneva & Rauh 2019), it is economically desirable to achieve more equality of educational opportunities. Therefore, this chapter shows that the implementation of an appropriate level of educational intensity, should not only depend on efficiency considerations, but also take into account the effects on equal access to resources.

Taking stock of this discussion, the third chapter of my dissertation shows that *circumstances* matter at school with an emphasis on the relevance of variation in **learning intensity** on **IEOp**. Future research should aim at understanding further potential mechanisms and channels shaping **IEOp** (Rothstein 2019). Furthermore, additional work is needed to establish how **IEOp** translates into social mobility. This in turn may then permit us to assess the welfare effects of **IEOp** with respect to its impact on future income and wealth inequality. Finally, the outcomes of this research agenda would allow for the evaluation of new policy recommendations aimed at improving equality of opportunity in order to tackle challenges surrounding high levels of inequality.

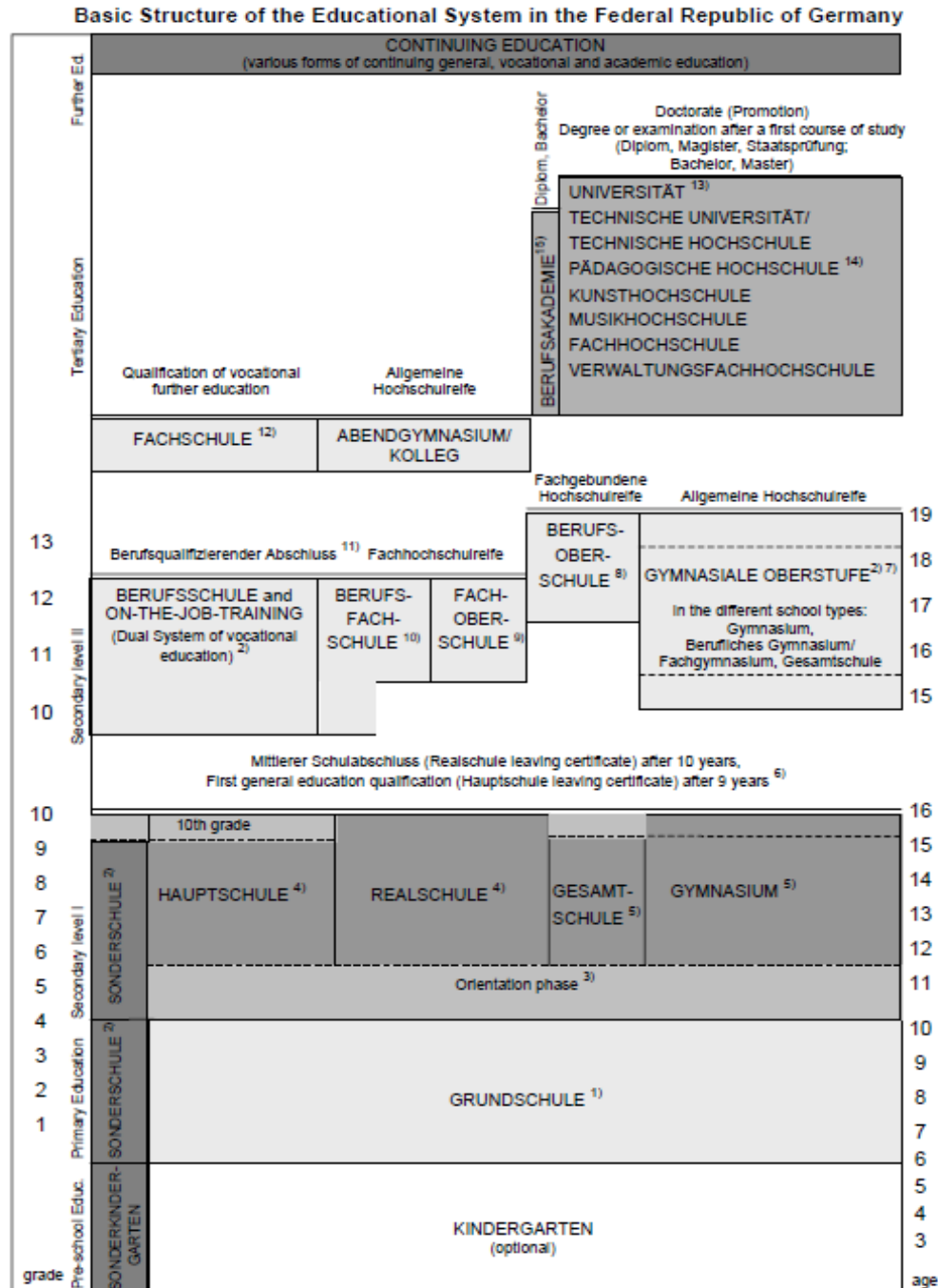
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<sup>34</sup>Thereby, a new theory of how learning (duration and intensity) and **IEOp** as well as how **IEOp** and social mobility are linked together could allow quantifying precisely the role of **learning intensity** for absolute educational mobility, thus social mobility.

### III.1 Appendix

#### III.1.1 Supplementary Figures

Figure III-1: Structure of the German Educational System



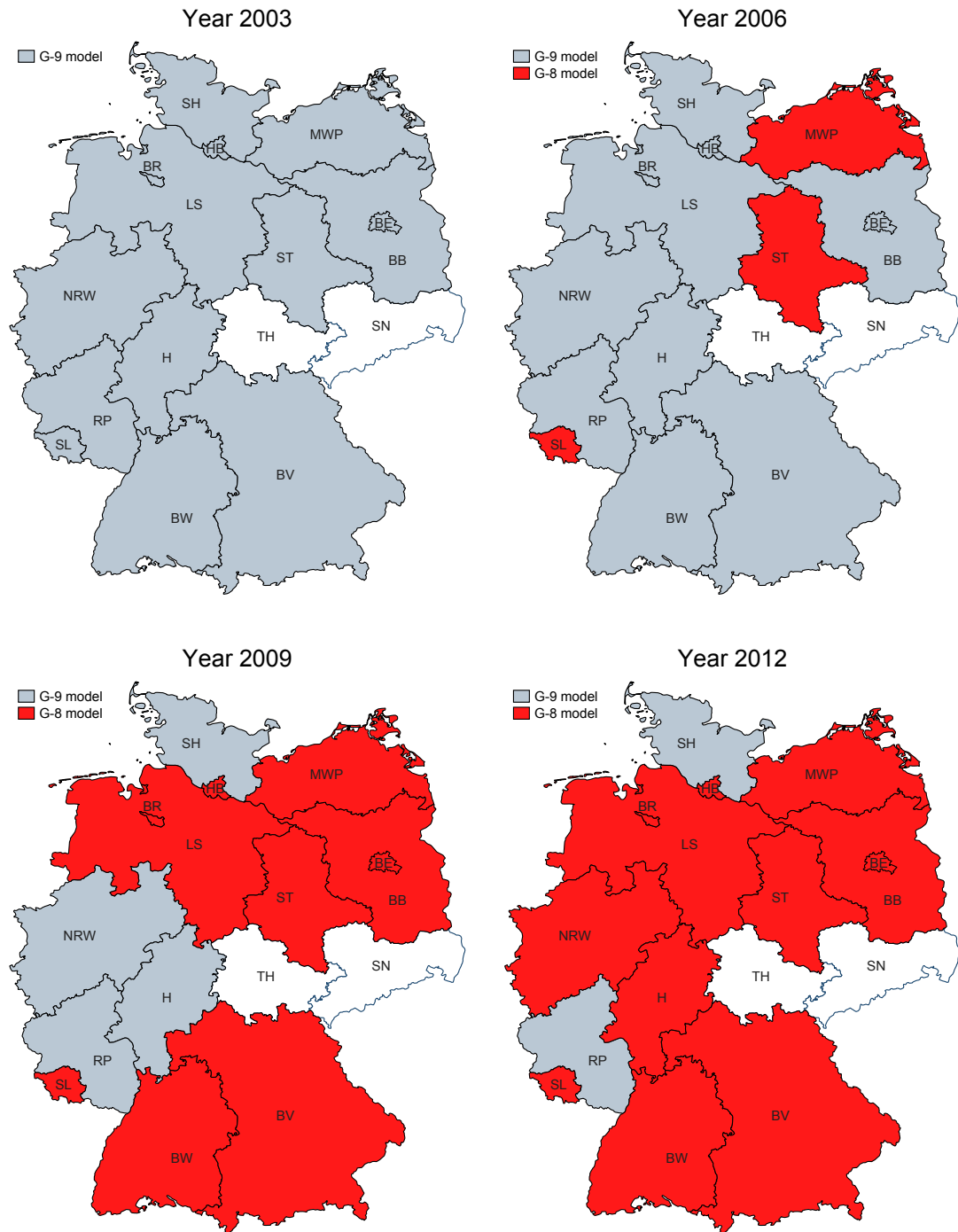
Published by: Secretariat of the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany, Documentation and Education Information Service, Lennéstr. 6, 53113 Bonn, Germany, Tel.+49 (0)228 501-0. © KMK 2009

**Notes:** This figure illustrates the basic structure of the German education system. For more details, see Standing Conference of Education Ministers (2009).

**Source:** Figure taken from Standing Conference of Education Ministers (2009): Basic Structure of the Education System in the Federal Republic of Germany.

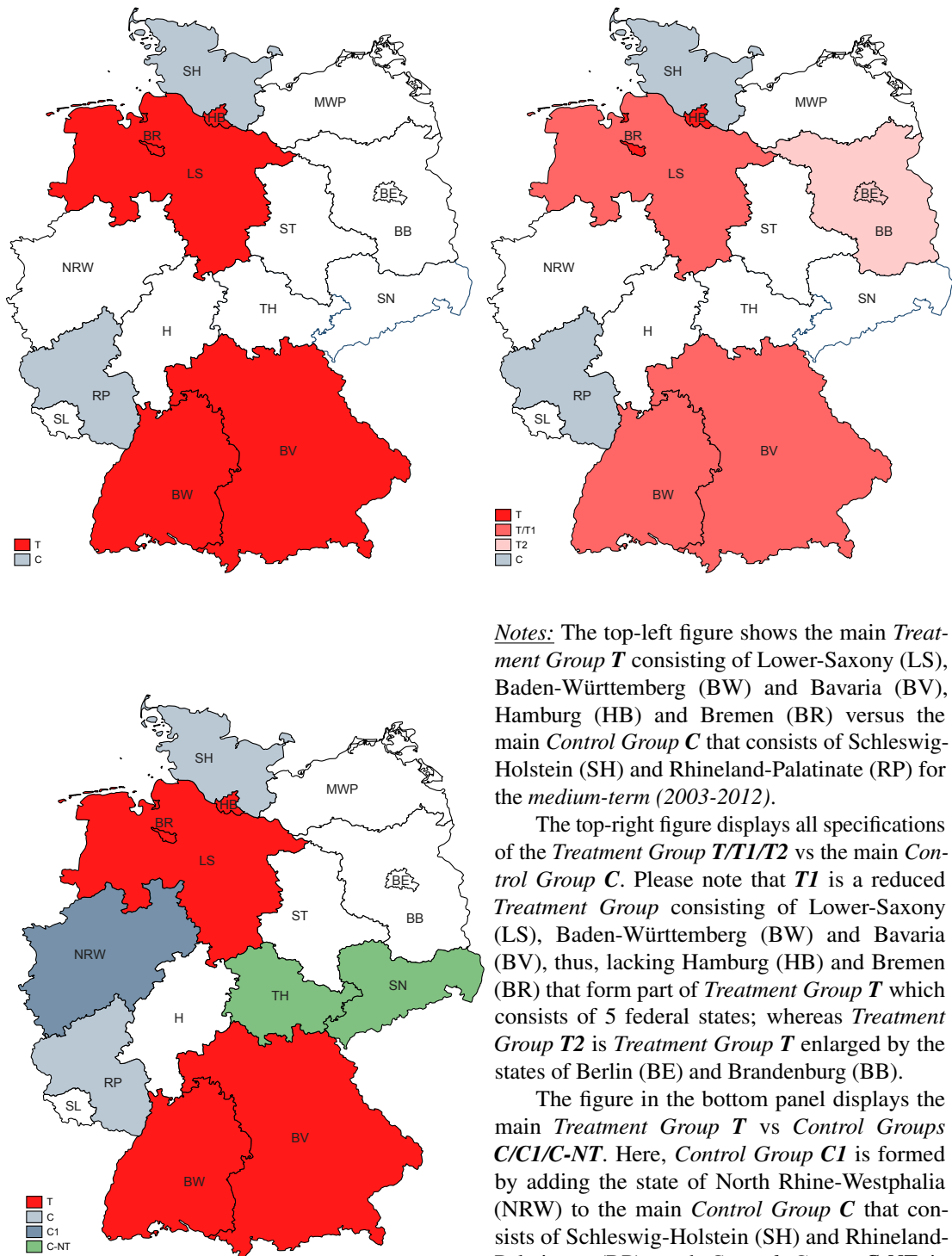


**Figure III-2:** Overview of G-8 Reform across Federal States for Students Tested in PISA (2003-2012)



*Notes:* This figure illustrates whether 9<sup>th</sup> graders attending a Gymnasium tested in a PISA test year (2003, 2006, 2009, 2012) were still taught in a G-9 model (grey/blue) or were already in a reformed G-8 model (dark grey/red).

Figure III-3: Overview of the Treatment/Control Group Setting

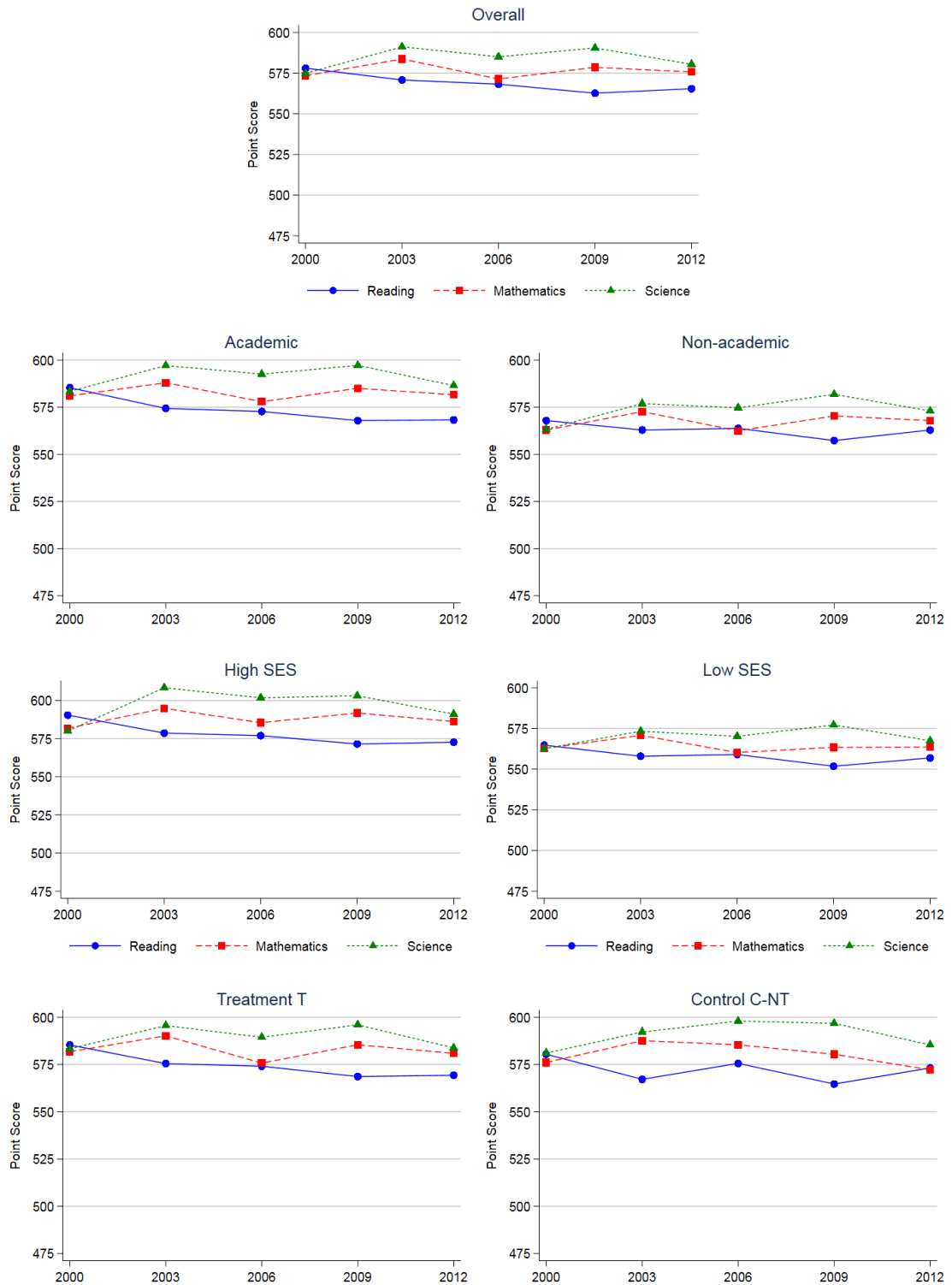


*Notes:* The top-left figure shows the main *Treatment Group T* consisting of Lower-Saxony (LS), Baden-Württemberg (BW) and Bavaria (BV), Hamburg (HB) and Bremen (BR) versus the main *Control Group C* that consists of Schleswig-Holstein (SH) and Rhineland-Palatinate (RP) for the *medium-term* (2003-2012).

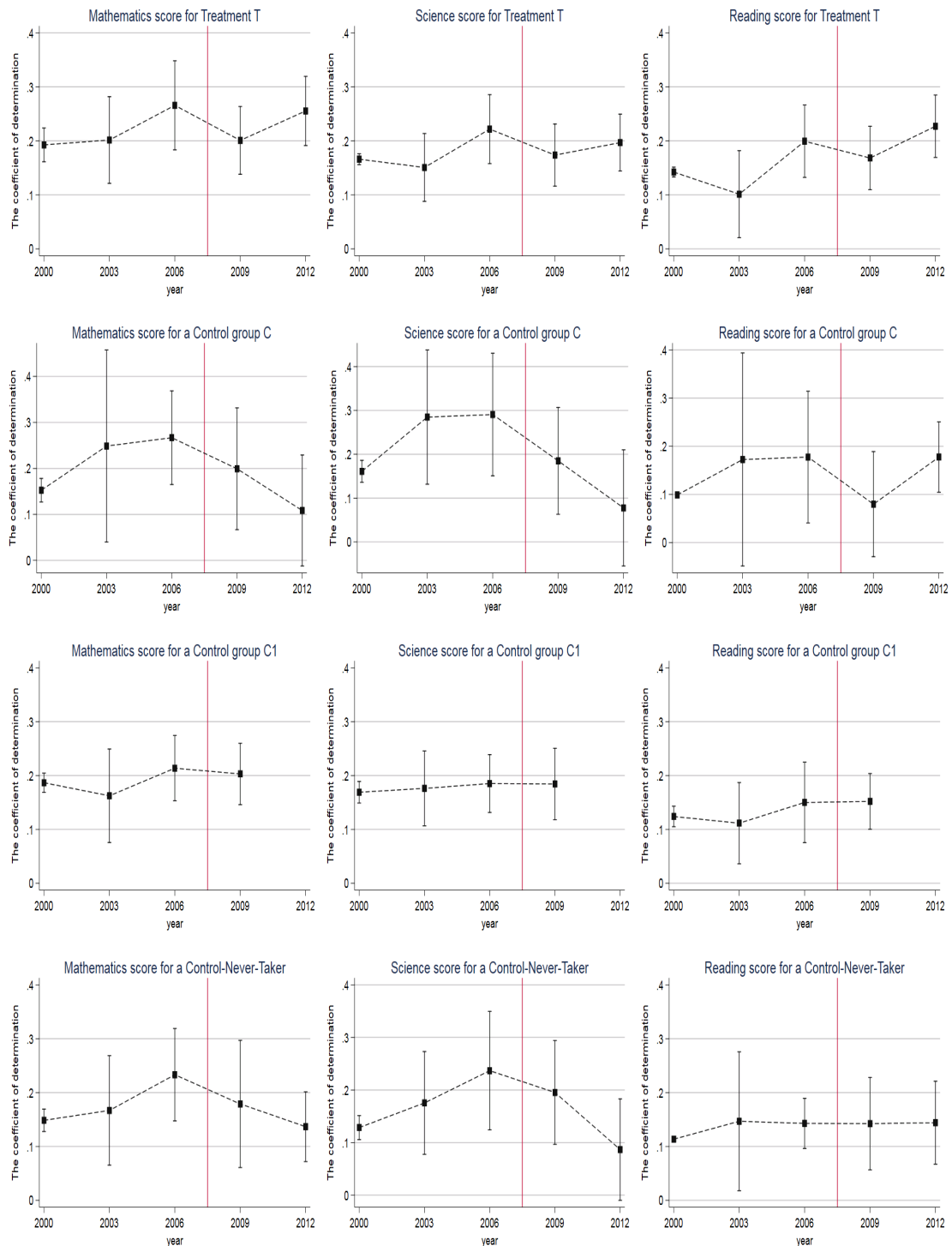
The top-right figure displays all specifications of the *Treatment Group T/T1/T2* vs the main *Control Group C*. Please note that *T1* is a reduced *Treatment Group* consisting of Lower-Saxony (LS), Baden-Württemberg (BW) and Bavaria (BV), thus, lacking Hamburg (HB) and Bremen (BR) that form part of *Treatment Group T* which consists of 5 federal states; whereas *Treatment Group T2* is *Treatment Group T* enlarged by the states of Berlin (BE) and Brandenburg (BB).

The figure in the bottom panel displays the main *Treatment Group T* vs *Control Groups C/C1/C-NT*. Here, *Control Group C1* is formed by adding the state of North Rhine-Westphalia (NRW) to the main *Control Group C* that consists of Schleswig-Holstein (SH) and Rhineland-Palatinate (RP), and *Control Group C-NT* is formed by adding Thuringia (TH) and Saxony (SN) to the main *Control Group C*.



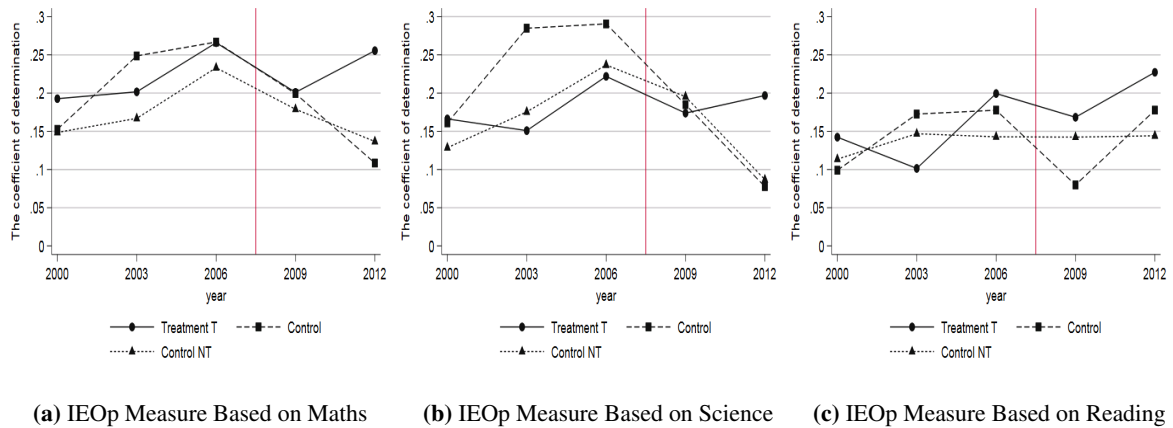
**Figure III-4: Descriptive Analysis: Mean Test Score by Main Groups**

*Notes:* This figure shows the mean scores for all three PISA test domains. Focusing on students in *Gymnasium*, the scores are above the average of 500 points. On overall, students perform best in science, then mathematics and relatively worst in reading. The grouping into academic vs. non-academic background is based on the binary variable indicating whether at least one parent has a college degree. To distinguish between high and low SES, students have been assigned to quartiles of their highest parental job's ISEI. Being in the first quartile translates into low SES, whereas the fourth quartile into high SES. Students from academic households achieve slightly higher scores than those from non-academic ones. A similar picture derives when distinguishing between high and low SES. Finally, the main *Treatment-Group T* and *Control-Group C-NT* (Never-Takers), as defined in Section 3.4.2, have similar test score levels.

**Figure III-5: IEOp Measure for Treatment/Control Groups Over Time (2000-2012)**

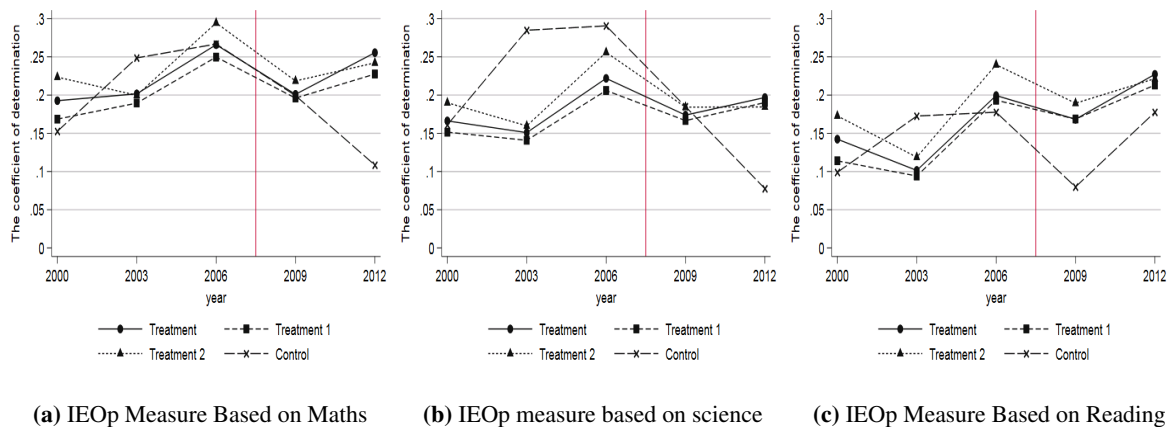
**Notes:** This figure shows the IEOp measure ( $R^2_{adjusted}$ ) with 90% confidence intervals over the whole time period. Standard errors to construct confidence intervals are calculated according to Appendix III.1.5. Standard errors for the year 2003 are particularly large due to idiosyncratic weights for that year.

**Source:** Author's own calculations based on PISA 2000, 2003, 2006, 2009 and 2012.

**Figure III-6: DiD Graphs of IEOP Measure for Main Treatment/Control Groups**

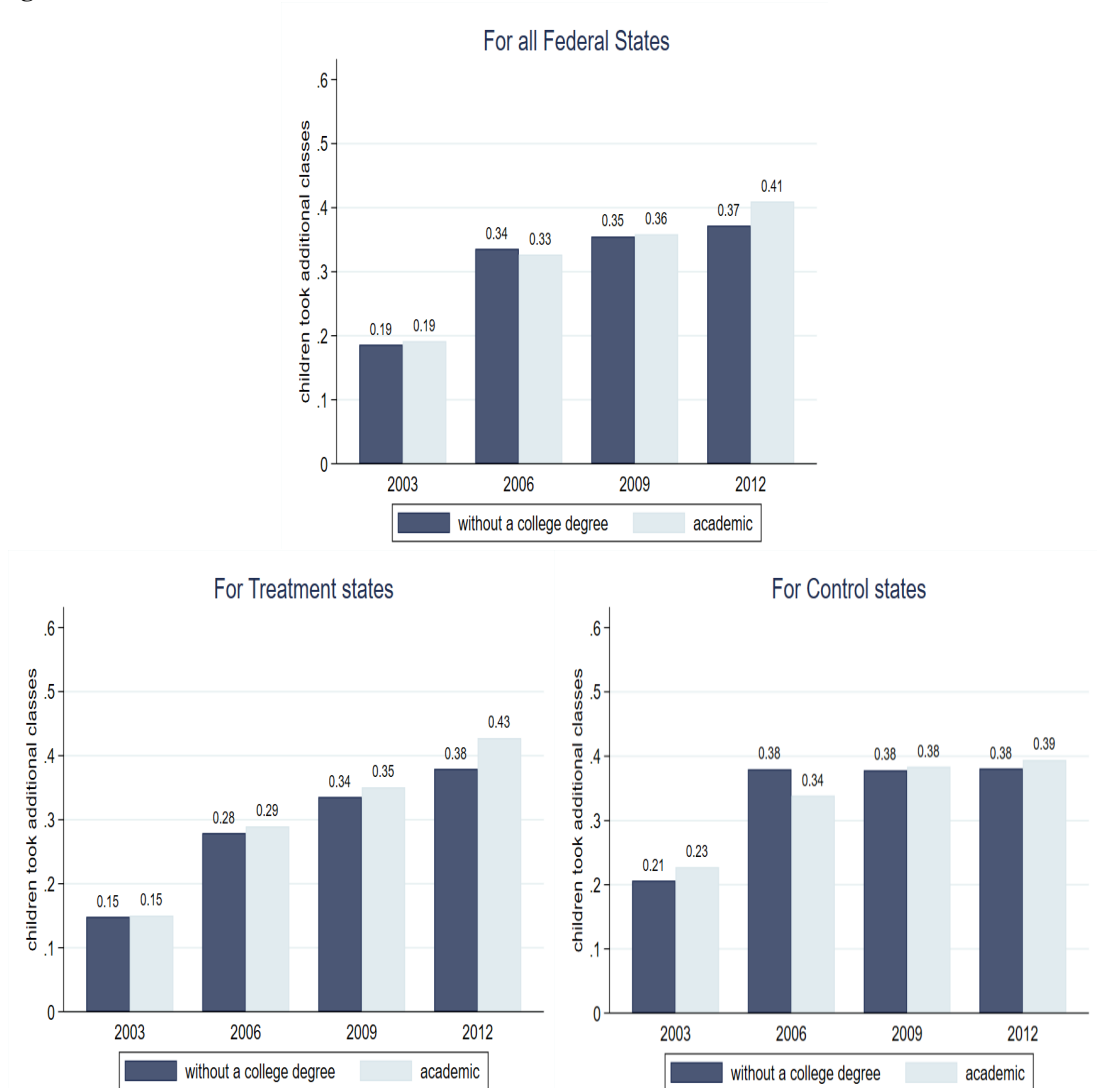
*Notes:* This figure shows the DiD graphs for all three test domains confirming the parallel trend assumption to hold. *Treatment* is the main treatment group **T**, *Control* is the main control group **C**, *Control-NT* is the never-changing control group.

*Source:* Author's own calculations based on PISA 2000, 2003, 2006, 2009 and 2012.

**Figure III-7: Robustness - DiD Graphs of IEOP Measure for Enlarged Treatment/Control Groups**

*Notes:* This figure shows the DiD graphs for all three test domains confirming the parallel trend assumption to hold and not being sensitive to alternative compositions of the treatment group. *Treatment* is the main treatment group **T** consisting of five federal states, *Treatment 1* is the **T1** Group consisting of three federal states, and *Treatment 2* is the extension Group consisting of seven federal states compared to *Control* which is the main control group **C**.

*Source:* Author's own calculations based on PISA 2000, 2003, 2006, 2009 and 2012.

**Figure III-8: Potential Mechanism: Extra Tuition**

*Notes:* This figure shows the percentage of tested students indicating that they took extra classes beyond official school lessons. This mostly includes paid extra tuition. The dark blue bars correspond to students growing up in non-academic households, whereas the light blue bars show results for students from academic households, i.e. growing up with at least one parent who has a university diploma (ISCED-level is greater than 5 or 6). The first panel shows that there was an upward trend in the demand for extra classes/tuition between 2003 and 2012 across all federal states. The second panel shows that in treatment states, the increase in extra tuition has been stronger for students from academic than non-academic households in the post-reform period from 2009 to 2012. This indicates that this differential adjustment with respect to extra-tuition depending on a student's parental educational background may explain the observed patterns in the main results. As in control groups, no such differential response in years 2009 and 2012 can be observed. Data are based on responses in the student questionnaires.

*Source:* Author's own calculations based on PISA 2003, 2006, 2009 and 2012.

### III.1.2 Supplementary Tables

**Table III.1:** Overview of "G-8 reform" Across Federal States by Year of *Double Cohort*

Federal state	Type of Federal State			Reform Timeline		Gymnasium		Reversal <sup>b</sup>
	West/East	City/Terr.	Population <sup>a</sup>	Begins	Ends	Type	Grade	yes/no
Saxony (SN)	East	territorial	4,0 mio	-	-	5-12	-	no reform <sup>c</sup>
Thuringia (TH)	East	territorial	2,2 mio	-	-	5-12	-	no reform <sup>c</sup>
Saxony-Anhalt (ST)	East	territorial	2,3 mio	2003/2004	2006/2007	5-12	9 <sup>th</sup>	no
Mecklenburg-West Pomerania (MWP)	East	territorial	1,6 mio	2004/2005	2007/2008	7-12	9 <sup>th</sup>	no
Saarland (SL)	West	territorial	1,0 mio	2001/2002	2008/2009	5-12	5 <sup>th</sup>	no <sup>d</sup>
Hamburg (HB)	West	city state	1,7 mio	2002/2003	2009/2010	5-12	5 <sup>th</sup>	no <sup>e</sup>
Bavaria (BV) <sup>f</sup>	West	territorial	12,5 mio	2004/2005	2010/2011	5-12	5 <sup>th</sup> , 6 <sup>th</sup>	yes <sup>g</sup>
Lower Saxony (LS) <sup>f</sup>	West	territorial	7,8 mio	2004/2005	2010/2011	5-12	5 <sup>th</sup> , 6 <sup>th</sup>	yes <sup>h</sup>
Baden-Württemberg (BW)	West	territorial	10,5 mio	2004/2005	2011/2012	5-12	5 <sup>th</sup>	no <sup>i</sup>
Bremen (BR)	West	city state	0,7 mio	2004/2005	2011/2012	5-12	5 <sup>th</sup>	no <sup>j</sup>
Berlin (BE)	West	city state	3,4 mio	2006/2007	2011/2012	7-12	7 <sup>th</sup>	no <sup>k</sup>
Brandenburg (BB)	East	territorial	2,5 mio	2006/2007	2011/2012	7-12	7 <sup>th</sup>	no <sup>k</sup>
North Rhine-Westphalia (NRW)	West	territorial	17,6 mio	2005/2006	2012/2013	5-12	5 <sup>th</sup>	no <sup>l</sup>
Hesse (H)	West	territorial	6,0 mio	varies <sup>m</sup>	varies <sup>m</sup>	5-12	5 <sup>th</sup>	yes <sup>n</sup>
Rhineland-Palatinate (RP)	West	territorial	4,0 mio	2008/2009	2015/2016	5-13	5 <sup>th</sup>	yes <sup>o</sup>
Schleswig-Holstein (SH)	West	territorial	2,8 mio	2008/2009	2015/2016	5-13	5 <sup>th</sup>	yes <sup>p</sup>

<sup>a</sup> Numbers taken from the most recent census in 2011 are valid for the considered time period from 2003 to 2012 (German Federal Statistical Office, 2014, Area and population).

<sup>b</sup> See Secretariat of Standing Conference of Ministers of Education:

<https://www.kmk.org/themen/allgemeinbildende-schulen/bildungswege-und-abschluesse/sekundarstufe-ii-gymnasiale-oberstufe-und-abitur.html>

<sup>c</sup> Since 1949, these states have implemented a **G-8 model** in the GDR and never had a **G-9 model**.

<sup>d</sup> Gymnasium remains in **G-8 model**, but in a comprehensive school G-13 model is possible.

<sup>e</sup> Gymnasium remains in **G-8 model**, whereas the Stadtschule as a comprehensive school offers a G-13 model.

<sup>f</sup> In Bavaria (BV) and Lower Saxony (LS), the 6<sup>th</sup> and 5<sup>th</sup> grade were allocated to the **G-8 model** in the same year, suggesting that educational intensity was stronger for then 6<sup>th</sup> graders who had to learn the curriculum over 7 instead of 8 years than (for then) 5<sup>th</sup> graders. Yet, tested 9<sup>th</sup> graders in 2009 were affected by the reform right from grade 5.

<sup>g</sup> General revision to **G-9 model** starting with school year 2019/2020 as announced in April 2017

<sup>h</sup> General revision to **G-9 model** starting with school year 2015/16, but with a voluntary option for the **G-8 model**

<sup>i</sup> But: since 2012/2013 a state-wide pilot project allows 44 model schools to offer a **G-9 model**.

<sup>j</sup> But: the so-called Oberschule as comprehensive school offers a G-13 model.

<sup>k</sup> But: integrated comprehensive schools are allowed to offer G-9 (G-13) model.

<sup>l</sup> But: in 2011/2012 there was a pilot project with 13/630 Gymnasien offering a **G-9 model**.

<sup>m</sup> Successive introduction of the reform in # % of all normal Gymnasium (5-12) 2004/2005: 10%; 2005/2006: 60%; 2006/2007: 30% with double cohorts graduating respectively in 2011/2012, 2012/2013 and 2013/2014.

<sup>n</sup> Since 2013/2014: students allowed to choose between G-12 or G-13 model from 5<sup>th</sup> grade onward.

<sup>o</sup> Always maintained schools with **G-9 model** (G-13 model): but since 2008/2009 a **G-8 model** is offered at 19 Gymnasien.

<sup>p</sup> Since 2011/12 schools are allowed by state law to offer a **G-9 model** (11 of 99 schools), **G-8 model** or both (4 of 99).

**Table III.2:** Available Grade-sample based PISA-I Datasets

Dataset	Before Reform			After Reform	
	PISA-2000 <sup>a</sup>	PISA-2003-I	PISA-2006-I	PISA-2009-I	PISA-2012-I
Student-dataset:					
# of variables	914	1,292	1,095	1,231	1,215
# of students <sup>b</sup>	34,754	8,559	9,577	9,460	9,998
test scores <sup>c</sup>	reading	mathematics	science	reading	mathematics
School-dataset:					
# of variables	470	572	565	534	502
# of schools	1,342	216	226	226	230
Teacher-dataset: <sup>d</sup>					
# of variables	-	653	-	639	257
# of teachers	-	1939	-	2,201	2,084

<sup>a</sup> For the year 2000, there was no specific *grade-based PISA-I-sample* available from the IQB. However, *PISA-2000* (being the *PISA-2000-E* data) is *ninth grade-based* (Baumert et al. 2002). It has a lower number of variables, but more observations than the other datasets.

<sup>b</sup> The number of observations for students as included in the *PISA* datasets (2000, 2003, 2006, 2009, 2012): the data is provided by the IQB and consists of the *grade-based sample* (see also Appendix III.1.4). Note, that here the *student-dataset* includes both the original students' questionnaire answers and their parental responses.

<sup>c</sup> These test score domains have been in focus for the respective *PISA* test cycle.

<sup>d</sup> For 2000 and 2006, the *teacher-dataset* was not part of the Germany-specific *PISA* data, as provided by the IQB.

**Table III.3:** Descriptive Statistics: Outcome Variables and Sample Size

Test Scores of Students in <i>Gymnasium</i>	Before Reform			After Reform	
	PISA-2000	PISA-2003-I	PISA-2006-I	PISA-2009-I	PISA-2012-I
Reading Mean	577.92	570.77	568.20	562.65	565.42
Reading SD	55.86	51.98	56.97	55.25	52.81
Reading Median	578.83	572.14	571.50	566.23	567.06
Mathematics Mean	573.65	583.66	571.39	578.53	575.73
Mathematics SD	62.18	57.85	58.48	56.59	58.52
Mathematics Median	572.68	584.70	571.19	580.47	576.19
Science Mean	575.14	591.15	585.01	590.48	580.44
Science SD	67.43	60.20	61.47	58.88	58.61
Science Median	576.35	594.80	587.12	594.68	581.07
# of federal states	16	16	16	16	16
# of schools	409	62	67	68	78
# of students	10,276	3,017	3,356	3,473	3,910

*Notes:* This table reports summary statistics for the sample of ninth graders attending a *Gymnasium* and is weighted by the sample weights provided in the *PISA* dataset from the IQB. Note that the average across plausible values can be taken as a metric of individual-level performance (further information on test scores and the weighting procedure is provided in Appendix III.1.4 and OECD (2012). Mean, standard deviations and median of the test scores across all federal states and for all academic track schools that are in the German *PISA* dataset are provided for each test cycle (2000, 2003, 2006, 2009, 2012) as shown in Table III.2.

**Table III.4:** Pre-Reform Treatment/Control Group Comparison of Control Variable Sets

	T	C	$\Delta$ (T-C)	T1	$\Delta$ (T1-C)	T2	$\Delta$ (T2-C)
<b>Individual Characteristics</b>							
Female	0.533	0.501	0.031	0.537	0.036	0.535	0.033
Age in years	15.495	15.475	0.020	15.491	0.016	15.478	0.003
Language at home not German	0.056	0.043	0.013	0.054	0.010	0.057	0.013
Migration Background	0.190	0.145	0.045**	0.184	0.039*	0.186	0.041*
<b>Parental Characteristics</b>							
Parental Education: (highest <b>ISCED</b> level)							
# <b>ISCED</b> -level (5-6):	0.664	0.644	0.019	0.666	0.021	0.670	0.025
# <b>ISCED</b> -level (3-4):	0.290	0.329	-0.040	0.290	-0.039	0.285	-0.045*
# <b>ISCED</b> -level (1-2):	0.046	0.026	0.02*	0.044	0.018	0.045	0.019*
<b>Socio-Economic Status</b>							
Number of books in household:							
# + 500:	0.233	0.235	-0.003	0.228	-0.008	0.223	-0.012
# 101-500:	0.509	0.520	-0.011	0.513	-0.007	0.504	-0.016
# 11-100:	0.204	0.189	0.015	0.206	0.017	0.215	0.026
# max. 10:	0.054	0.055	-0.001	0.052	-0.003	0.058	0.002
Highest <b>ISEI</b> of parental job	59.427	57.072	2.355***	59.322	2.25**	59.109	2.037**
<b>Family Characteristics</b>							
Single Parent ( <i>Base cat.: No</i> )	0.140	0.141	-0.001	0.137	-0.004	0.168	0.027
Father employment status							
# full-time (FT):	0.875	0.866	0.008	0.875	0.008	0.864	-0.002
# part-time (PT):	0.067	0.065	0.002	0.066	0.001	0.067	0.002
# unemployed (UE):	0.026	0.033	-0.007	0.025	-0.008	0.036	0.003
# out-of-labor force (OLF) :	0.032	0.036	-0.003	0.034	-0.002	0.033	-0.003
Mother employment status							
# full-time (FT):	0.220	0.218	0.002	0.220	0.002	0.303	0.084***
# part-time (PT):	0.521	0.513	0.007	0.522	0.008	0.457	-0.057**
# unemployed (UE):	0.061	0.077	-0.016	0.061	-0.015	0.068	-0.008
# out-of-labor force (OLF):	0.198	0.192	0.006	0.197	0.005	0.172	-0.019
Number of students	2,365	347	-	2,175	-	2,999	-

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10%. This table shows a *two-sample t-test* for comparing the main control variables in the pre-reform period of the main specification between *Treatment* and *Control Group* (see [Section 3.3.3](#) and [Section 3.4.1](#)). This is for *PISA-I* the respective pooled average of control variables for 2003 and 2006.

*Source:* Author's calculations based on **PISA** 2003 and 2006.

**Table III.5: Robust Model: Pre-Reform Treatment/Control Group Comparison of Control Variables**

	T	C1	$\Delta$ (T-C1)	T1	$\Delta$ (T1-C1)	T2	$\Delta$ (T2-C1)
<b>Individual Characteristics</b>							
Female	0.533	0.549	-0.016	0.537	-0.011	0.535	-0.014
Age in years	15.495	15.468	0.028*	15.491	0.024	15.478	0.011
Language at home not German	0.056	0.056	0.000	0.054	-0.002	0.057	0.001
Migration Background	0.190	0.178	0.012	0.184	0.006	0.186	0.008
<b>Parental Characteristics</b>							
Parental Education: (highest <b>ISCED</b> level)							
# <b>ISCED</b> -level (5-6):	0.664	0.666	-0.003	0.666	-0.001	0.670	0.003
# <b>ISCED</b> -level (3-4):	0.290	0.296	-0.007	0.290	-0.006	0.285	-0.012
# <b>ISCED</b> -level (1-2):	0.046	0.037	0.009	0.044	0.007	0.045	0.008
<b>Socio-Economic Status</b>							
Number of books in household:							
# + 500:	0.233	0.253	-0.021	0.228	-0.026*	0.223	-0.030**
# 101-500:	0.509	0.496	0.013	0.513	0.018	0.504	0.008
# 11-100:	0.204	0.197	0.007	0.206	0.009	0.215	0.018
# max. 10:	0.054	0.054	0.000	0.052	-0.001	0.058	0.004
Highest <b>ISEI</b> of parental job	59.427	58.818	0.609	59.322	0.503	59.109	0.291
<b>Family Characteristics</b>							
Single Parent ( <i>Base cat.: No</i> )	0.140	0.150	-0.010	0.137	-0.013	0.168	0.018
Father employment status							
# full-time (FT):	0.875	0.878	-0.004	0.875	-0.004	0.864	-0.014
# part-time (PT):	0.067	0.061	0.007	0.066	0.006	0.067	0.007
# unemployed (UE):	0.026	0.027	-0.001	0.025	-0.002	0.036	0.009*
# out-of-labor force (OLF) :	0.032	0.034	-0.002	0.034	0.000	0.033	-0.002
Mother employment status							
# full-time (FT):	0.220	0.239	-0.018	0.220	-0.019	0.303	0.064***
# part-time (PT):	0.521	0.489	0.032**	0.522	0.033**	0.457	-0.032**
# unemployed (UE):	0.061	0.065	-0.004	0.061	-0.003	0.068	0.004
# out-of-labor force (OLF):	0.198	0.208	-0.010	0.197	-0.011	0.172	-0.035***
Number of students	2,365	1854	-	2,175	-	2,999	-

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. This table shows a *two-sample t-test* for comparing the main control variables in the pre-reform period between *Treatment Groups* and *Control Group C1* (see [Section 3.3.3](#) and [Section 3.4.1](#)). This is for *PISA-I* the respective pooled average of control variables for 2003 and 2006.

*Source:* Author's calculations based on *PISA* 2003 and 2006.



**Table III.6:** Main Results for *Model Base*: 1<sup>st</sup> step to Derive IEOp Measure for *Mathematics*

DEPENDENT VARIABLE:		Control Group (C)		Treatment Group (T)	
Mathematics Test Scores in PISA (STDPVMATH3)		Before (2003-2006)	After (2009-2012)	Before (2003-2006)	After (2009-2012)
CONTROL: Individual Characteristics (IC)					
i)	Female	-0.662*** (0.116)	-0.533*** (0.066)	-0.464*** (0.044)	-0.450*** (0.046)
	Age in years	-0.110 (0.112)	-0.313*** (0.072)	-0.209*** (0.045)	-0.220*** (0.037)
ii)	Migration Background	-0.084 (0.185)	-0.127 (0.135)	-0.128* (0.071)	-0.162*** (0.048)
	NO German spoken at home	-0.373 (0.337)	-0.170 (0.191)	-0.082 (0.104)	-0.198*** (0.077)
CONTROL: Parental Characteristics (PC)					
iii)	Parental Education: [Base: <i>ISCED</i> -level (3-4)] # at most lower sec. educ. ( <i>ISCED</i> -level (1-2))	-0.454* (0.275)	0.133 (0.152)	-0.169** (0.080)	-0.092 (0.069)
	# tertiary educ. ( <i>ISCED</i> -level (5-6))	-0.230** (0.101)	0.057 (0.148)	0.020 (0.045)	-0.001 (0.037)
CONTROL: Socio-Economic Status (SES)					
iv)	No. of books in household [Base: 101-500] # max 10 books	0.342 (0.243)	-0.411 (0.305)	-0.398** (0.155)	-0.316*** (0.103)
	# 11-100 books	-0.120 (0.078)	-0.078 (0.129)	-0.253*** (0.061)	-0.134*** (0.046)
	# more than 500 books	0.234 (0.149)	0.185 (0.120)	0.074 (0.056)	0.116*** (0.037)
v)	Highest <i>ISEI</i> -level of Parental Jobs	0.007 (0.005)	0.002 (0.002)	0.002 (0.002)	0.004*** (0.001)
CONTROL: Family Characteristics (FC)					
vi)	Family Structure [Base: <i>No</i> ] single parent household	0.058 (0.176)	0.195* (0.103)	0.046 (0.054)	0.112** (0.052)
vii)	Father: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	-0.238 (0.278)	-0.413* (0.218)	0.007 (0.085)	-0.117* (0.061)
	# unemployed (UE)	0.075 (0.353)	0.100 (0.428)	-0.210 (0.138)	0.109 (0.139)
	# out-of-labor force (OLF)	-0.044 (0.308)	-0.174 (0.146)	-0.026 (0.143)	-0.028 (0.106)
	Mother: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	0.096 (0.068)	0.025 (0.125)	-0.001 (0.065)	0.045 (0.052)
	# unemployed (UE)	0.212 (0.161)	0.143 (0.597)	-0.003 (0.080)	0.232** (0.092)
	# out-of-labor force (OLF)	-0.072 (0.154)	0.203 (0.125)	-0.037 (0.083)	0.082 (0.072)
Constant		1.824 (1.755)	4.853*** (1.121)	3.581*** (0.711)	3.539*** (0.583)
School FE		yes	yes	yes	yes
Observations		346	608	2356	3329
R <sup>2</sup>		0.353*** (0.060)	0.189*** (0.041)	0.267*** (0.033)	0.248*** (0.026)
R <sup>2</sup> – <i>adjusted</i>		0.294*** (0.065)	0.144*** (0.043)	0.244*** (0.034)	0.228*** (0.027)

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. This table shows the first stage OLS regressions to derive the  $R^2$  as **IEOp** measure for conducting the **DiD** estimation approach, with the results shown in the second sub-panel in **Table III.11**. The dependent variable is *stdpvmath3*, i.e. **standardized PISA mathematics test scores** for each test year with respect to students in **Gymnasium** that are part of the representative *grade-based* German **PISA** test cohort across the respective *Model Base* period (2003-2012) (**Footnote 25**). Columns (1) to (2) showing the results for Control Group (C) provide first-step regressions for the *Before-reform period* (2003-2006) in column 1 and *After-reform period* (2009-2012) in column 2. Columns (3) to (4) provide first-step regression results for Treatment Group (T) with *Before-reform period* (2003-2006) results in column 3 and *After-reform period* (2009-2012) results in column 4. Background variables used to derive  $R^2$  are explained in **Section 3.3.3** and listed in four groups with a total of seven subgroups (compare **Appendix III.1.5**). Observations are weighted according to the provided **population weights**. Standard errors are clustered at the federal state level, and inflated by the estimated measurement error in test scores (compare **Appendix III.1.5** on their computation).

*Source:* Author's calculations based on **PISA** 2003, 2006, 2009 and 2012.

**Table III.7:** Main Results for *Model Base*: 1<sup>st</sup> step to Derive IEOp Measure for *Reading*

DEPENDENT VARIABLE:		Control Group (C)		Treatment Group (T)	
Reading Test Scores in PISA (STDPVREAD3)		Before (2003-2006)	After (2009-2012)	Before (2003-2006)	After (2009-2012)
CONTROL: Individual Characteristics (IC)					
i)	Female	0.066 (0.105)	0.393*** (0.086)	0.288*** (0.040)	0.412*** (0.038)
	Age in years	-0.059 (0.183)	-0.248** (0.119)	-0.167** (0.065)	-0.159*** (0.040)
ii)	Migration Background	-0.234 (0.241)	-0.167* (0.090)	-0.074 (0.073)	-0.105* (0.055)
	NO German spoken at home	-0.494 (0.530)	-0.153 (0.201)	-0.303*** (0.113)	-0.168** (0.071)
CONTROL: Parental Characteristics (PC)					
iii)	Parental Education: [Base: <i>ISCED</i> -level (3-4)]				
	# at most lower sec. educ. ( <i>ISCED</i> -level (1-2))	-0.486** (0.225)	0.107 (0.199)	-0.303*** (0.100)	-0.005 (0.055)
	# tertiary educ. ( <i>ISCED</i> -level (5-6))	-0.159 (0.156)	0.134 (0.109)	-0.009 (0.057)	-0.048 (0.048)
CONTROL: Socio-Economic Status (SES)					
iv)	No. of books in household [Base: 101-500]				
	# max 10 books	0.169 (0.395)	-0.441 (0.272)	-0.522** (0.225)	-0.441*** (0.125)
	# 11-100 books	-0.126 (0.214)	-0.079 (0.120)	-0.303*** (0.053)	-0.138*** (0.043)
	# more than 500 books	0.204* (0.117)	0.077 (0.069)	0.079 (0.060)	0.087* (0.051)
v)	Highest <i>ISEI</i> -level of Parental Jobs	0.007 (0.005)	0.002 (0.003)	0.001 (0.002)	0.003*** (0.001)
CONTROL: Family Characteristics (FC)					
vi)	Family Structure [Base: <i>No</i> ] Single Parent Household	0.079 (0.261)	0.268** (0.122)	0.066 (0.066)	0.127** (0.057)
vii)	Father: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	-0.300 (0.208)	-0.233 (0.280)	-0.089 (0.095)	-0.096 (0.074)
	# unemployed (UE)	0.382 (0.441)	0.320 (0.373)	-0.023 (0.187)	0.106 (0.154)
	# out-of-labor force (OLF)	0.014 (0.271)	-0.079 (0.182)	0.082 (0.150)	0.125 (0.097)
	Mother: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	-0.013 (0.120)	-0.058 (0.107)	0.005 (0.058)	0.004 (0.045)
	# unemployed (UE)	0.267 (0.240)	0.257 (0.490)	-0.062 (0.127)	0.136 (0.097)
	# out-of-labor force (OLF)	-0.165 (0.150)	0.101 (0.120)	-0.053 (0.079)	-0.023 (0.058)
	Constant	0.754 (2.969)	3.348* (1.874)	2.611** (1.054)	2.141*** (0.638)
	School FE	yes	yes	yes	yes
Observations		346	608	2356	3329
R <sup>2</sup>		0.242*** (0.057)	0.162*** (0.034)	0.180*** (0.031)	0.213*** (0.020)
R <sup>2</sup> – <i>adjusted</i>		0.172*** (0.062)	0.114*** (0.036)	0.154*** (0.032)	0.192*** (0.021)

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. This table shows the first stage OLS regressions to derive the R<sup>2</sup> as IEOp measure for conducting the DiD estimation approach, with the results shown in the first sub-panel in Table III.11. The dependent variable is *stdpvread3*, i.e. **standardized PISA reading test scores** for each test year with respect to students in *Gymnasium* that are part of the representative *grade-based* German **PISA** test cohort across the respective *Model Base* period (2003-2012) (Footnote 25). Columns (1) to (2) showing the results for Control Group (C) provide first-step regressions for the *Before-reform period* (2003-2006) in column 1 and *After-reform period* (2009-2012) in column 2. Columns (3) to (4) provide first-step regression results for Treatment Group (T) with *Before-reform period* (2003-2006) results in column 3 and *After-reform period* (2009-2012) results in column 4. Background variables used to derive R<sup>2</sup> are explained in Section 3.3.3 and listed in four groups with a total of seven subgroups (compare Appendix III.1.5). Observations are weighted according to the provided **population weights**. Standard errors are clustered at the federal state level, and inflated by the estimated measurement error in test scores (compare Appendix III.1.5 on their computation).

*Source:* Author's calculations based on **PISA** 2003, 2006, 2009 and 2012.

**Table III.8:** Main Results for *Model Base*: 1<sup>st</sup> step to Derive IEOp Measure for *Science Scores*

DEPENDENT VARIABLE:		Control Group (C)		Treatment Group (T)	
Science Test Scores in PISA (STDPVSCIE3)		Before (2003-2006)	After (2009-2012)	Before (2003-2006)	After (2009-2012)
CONTROL: Individual Characteristics (IC)					
i)	Female	-0.509*** (0.113)	-0.340*** (0.064)	-0.354*** (0.037)	-0.287*** (0.040)
	Age in years	-0.093 (0.118)	-0.252*** (0.082)	-0.163*** (0.062)	-0.160*** (0.053)
ii)	Migration Background	-0.297* (0.163)	-0.287*** (0.096)	-0.087 (0.082)	-0.202*** (0.054)
	NO German spoken at home	-0.347 (0.403)	-0.158 (0.221)	-0.222** (0.092)	-0.195*** (0.067)
CONTROL: Parental Characteristics (PC)					
iii)	Parental Education: [Base: <i>ISCED-level (3-4)</i> ]				
	# at most lower sec. educ. ( <i>ISCED-level (1-2)</i> )	-0.529** (0.243)	0.076 (0.184)	-0.259*** (0.092)	-0.047 (0.061)
	# tertiary educ. ( <i>ISCED-level (5-6)</i> )	-0.153 (0.123)	0.116 (0.126)	0.030 (0.055)	0.005 (0.040)
CONTROL: Socio-Economic Status (SES)					
iv)	No. of books in household [Base: <i>101-500</i> ]				
	# max 10 books	0.101 (0.413)	-0.397 (0.281)	-0.306** (0.149)	-0.525*** (0.068)
	# 11-100 books	-0.122 (0.106)	-0.114 (0.112)	-0.282*** (0.067)	-0.200*** (0.045)
	# more than 500 books	0.175 (0.147)	0.120 (0.111)	0.149** (0.060)	0.163*** (0.038)
v)	Highest <i>ISEI</i> -level of Parental Jobs	0.009** (0.004)	0.003 (0.003)	0.003* (0.002)	0.003** (0.002)
CONTROL: Family Characteristics (FC)					
vi)	Family Structure [Base: <i>No</i> ] Single Parent Household	0.051 (0.241)	0.207** (0.084)	0.008 (0.076)	0.122* (0.063)
vii)	Father: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	-0.162 (0.207)	-0.223 (0.190)	-0.054 (0.116)	-0.141* (0.074)
	# unemployed (UE)	0.192 (0.426)	0.161 (0.378)	-0.022 (0.125)	0.091 (0.166)
	# out-of-labor force (OLF)	0.036 (0.290)	-0.021 (0.238)	0.019 (0.129)	0.068 (0.098)
	Mother: Employment [Base: <i>Full-time (FT)</i> ] # part-time (PT)	-0.083 (0.097)	-0.059 (0.098)	-0.016 (0.064)	0.011 (0.046)
	# unemployed (UE)	0.184 (0.156)	0.101 (0.307)	-0.021 (0.101)	0.195* (0.102)
	# out-of-labor force (OLF)	-0.218 (0.139)	0.087 (0.113)	-0.089 (0.067)	0.006 (0.059)
	Constant	1.593 (1.918)	3.933*** (1.287)	2.784*** (1.006)	2.614*** (0.803)
	School FE	yes	yes	yes	yes
Observations		346	608	2356	3329
R <sup>2</sup>		0.363*** (0.052)	0.173*** (0.048)	0.214*** (0.025)	0.209*** (0.023)
R <sup>2</sup> – <i>adjusted</i>		0.304*** (0.057)	0.125** (0.051)	0.190*** (0.026)	0.188*** (0.023)

*Notes:* Robust standard errors are shown in parentheses and significance levels are indicated by: \*\*\* 1%, \*\* 5%, \* 10% + 15%. This table shows the first stage OLS regressions to derive the *R*<sup>2</sup> as *IEOp* measure for conducting the *DiD* estimation approach, with the results shown in the third sub-panel in [Table III.11](#). The dependent variable is *stdpvsie3*, i.e. **standardized PISA science test scores** for each test year with respect to students in *Gymnasium* that are part of the representative *grade-based* German *PISA* test cohort across the respective *Model Base* period (2003-2012) ([Footnote 25](#)). Columns (1) to (2) showing the results for Control Group (C) provide first-step regressions for the *Before-reform period* (2003-2006) in column 1 and *After-reform period* (2009-2012) in column 2. Columns (3) to (4) provide first-step regression results for Treatment Group (T) with *Before-reform period* (2003-2006) results in column 3 and *After-reform period* (2009-2012) results in column 4. Background variables used to derive *R*<sup>2</sup> are explained in [Section 3.3.3](#) and listed in four groups with a total of seven subgroups (compare [Appendix III.1.5](#)). Observations are weighted according to the provided **population weights**. Standard errors are clustered at the federal state level, and inflated by the estimated measurement error in test scores (compare [Appendix III.1.5](#) on their computation).

*Source:* Author's calculations based on *PISA* 2003, 2006, 2009 and 2012.

**Table III.9:** *Robust Model* for T vs. C and C1

Subject	Main Control Group C			Extended Control Group C1		
	C	T	$\Delta$ (T-C)	C1	T	$\Delta$ (T-C1)
<b>Reading</b>						
Before	0.242 (0.057)	0.180 (0.031)	-0.062 (0.065)	0.163 (0.032)	0.180 (0.031)	0.016 (0.044)
After	0.161 (0.060)	0.195 (0.034)	0.035 (0.069)	0.183 (0.030)	0.195 (0.034)	0.012 (0.046)
Change in $R^2$	-0.081 (0.083)	0.016 (0.046)	<b>0.097</b> (0.095)	0.020 (0.044)	0.016 (0.046)	<b>-0.004</b> (0.064)
<b>Mathematics</b>						
Before	0.353 (0.060)	0.267 (0.033)	-0.086 (0.068)	0.216 (0.029)	0.267 (0.033)	0.052 (0.044)
After	0.270 (0.073)	0.227 (0.037)	-0.043 (0.082)	0.233 (0.033)	0.227 (0.037)	-0.006 (0.050)
Change in $R^2$	-0.084 (0.094)	-0.040 (0.049)	<b>0.043</b> (0.107)	0.017 (0.044)	-0.040 (0.049)	<b>-0.057</b> (0.066)
<b>Science</b>						
Before	0.363 (0.052)	0.215 (0.025)	-0.148 (0.058)	0.205 (0.024)	0.215 (0.025)	0.010 (0.035)
After	0.257 (0.067)	0.201 (0.034)	-0.056 (0.075)	0.215 (0.039)	0.201 (0.034)	-0.014 (0.051)
Change in $R^2$	-0.106 (0.085)	-0.014 (0.042)	<b>0.092</b> (0.095)	0.010 (0.046)	-0.014 (0.042)	<b>-0.024</b> (0.062)

*Notes:* Table entries are  $R^2$  measures of **IEOp** (Equation (3.7)). Robust standard errors are in parentheses and were calculated using replication weights following the method as explained in Appendix III.1.5, clustering at the federal state level. **DiD** results are estimated according to Equation (3.9) taking into account population weights. Positive changes in  $R^2$  indicate increasing **IEOp** or decreasing **EEOp** and vice versa for negative changes.

*Background variables used to derive  $R^2$ :*

- (i) Individual Characteristics (IC) I: *age and gender*
- (ii) Individual Characteristics (IC) II: *language spoken at home; migration background* (based on par. birth place)
- (iii) Parental Characteristics (PC): *highest parents' qualification* (*ISCED*-level 1-2/*ISCED* 3-4/*ISCED* 5-6)
- (iv) Socio-economic Status (**SES**) I: *no. of books in household* (max. 11, 11-100, 101-500, more than 500)
- (v) Socio-economic Status (**SES**) II : *highest ISEI-level-index [0-90] of job in the family*
- (vi) Family Characteristics (FC) I: *family structure - growing up in single parent household?*
- (vii) Family Characteristics (FC) II: *mother/father working part-time (PT) - mother/father unemployed (UE) - mother/father out of labor force (OLF)*

*Compare:* The first-step regressions of the setting: treatment group T vs. control group C are provided in Table III.7, Table III.6 and Table III.8 in Appendix III.1.2.

*Source:* Author's calculations based on PISA 2003, 2006 and 2009 (compare Section 3.3.1).

**Table III.10:** Robustness Checks: Placebo Tests (2003-2006) T vs. C

Subject	With $R^2$ Measure			With $R^2_{adjusted}$ Measure		
	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
<b>Reading</b>						
Before (2003)	0.288 (0.115)	0.139 (0.047)	-0.149 (0.125)	0.173 (0.134)	0.101 (0.049)	-0.071 (0.143)
After (2006)	0.284 (0.072)	0.229 (0.039)	-0.055 (0.082)	0.178 (0.083)	0.199 (0.041)	0.022 (0.092)
Change in $R^2$	-0.004 (0.136)	0.090 (0.061)	<b>0.094</b> (0.149)	0.005 (0.158)	0.098 (0.064)	<b>0.093</b> (0.170)
<b>Mathematics</b>						
Before (2003)	0.353 (0.109)	0.235 (0.047)	-0.118 (0.119)	0.249 (0.127)	0.202 (0.049)	-0.047 (0.136)
After (2006)	0.362 (0.054)	0.293 (0.048)	-0.069 (0.072)	0.267 (0.062)	0.266 (0.050)	-0.001 (0.079)
Change in $R^2$	0.009 (0.122)	0.058 (0.067)	<b>0.049</b> (0.139)	0.018 (0.141)	0.064 (0.070)	<b>0.046</b> (0.157)
<b>Science</b>						
Before (2003)	0.384 (0.080)	0.186 (0.037)	-0.198 (0.088)	0.285 (0.093)	0.151 (0.038)	-0.134 (0.100)
After (2006)	0.383 (0.074)	0.251 (0.037)	-0.132 (0.083)	0.291 (0.085)	0.222 (0.039)	-0.069 (0.093)
Change in $R^2$	-0.002 (0.109)	0.064 (0.052)	<b>0.066</b> (0.121)	0.006 (0.126)	0.071 (0.054)	<b>0.065</b> (0.137)

*Notes:* Table entries are  $R^2$  measures of **IEOp** (Equation (3.7)). Robust standard errors are in parentheses and were calculated using replication weights following the method as explained in Appendix III.1.5, clustering at the federal state level. **DiD** results are estimated according to Equation (3.9) taking into account population weights and the indicated school fixed effects. Positive changes in  $R^2$  indicate increasing **IEOp** or decreasing **EEOp** and vice versa for negative changes.

*Background variables used to derive  $R^2$ :*

- (i) Individual Characteristics (IC) I: *age* and *gender*
- (ii) Individual Characteristics (IC) II: *language spoken at home*; *migration background* (based on par. birth place)
- (iii) Parental Characteristics (PC): *highest parents' qualification* (**ISCED**-level 1-2/**ISCED** 3-4/**ISCED** 5-6)
- (iv) Socio-economic Status (**SES**) I: *no. of books in household* (max. 11, 11-100, 101-500, more than 500)
- (v) Socio-economic Status (**SES**) II: *highest **ISEI**-level-index[0-90] of job in the family*
- (vi) Family Characteristics (FC) I: *family structure - growing up in single parent household?*
- (vii) Family Characteristics (FC) II: *mother/father working part-time (PT) - mother/father unemployed (UE) - mother/father out of labor force (OLF)*

*Compare:* Due to space constraints first-step regressions for T vs. C/C1/C2 have been omitted, but they are available upon request from the author.

*Source:* Author's calculations based on **PISA** 2003 and 2006.

**Table III.11:** Robustness Check of Main Results: Testing Potential Sorting across Schools

Subject	<i>Model Base (2003-2012) - T vs. C — (Figure III-3)</i>					
	Main: School Fixed Effects			Robustness: State Fixed Effects		
Reading	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.242 (0.057)	0.180 (0.031)	-0.062 (0.065)	0.180 (0.054)	0.121 (0.025)	-0.059 (0.060)
After	0.162 (0.034)	0.213 (0.020)	0.051 (0.039)	0.131 (0.034)	0.140 (0.019)	0.009 (0.039)
Change in $R^2$	-0.080 (0.066)	0.033 (0.037)	<b>0.113</b> (0.076)	-0.049 (0.064)	0.019 (0.032)	<b>0.068</b> (0.071)
Mathematics	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.353 (0.060)	0.267 (0.033)	-0.086 (0.068)	0.300 (0.059)	0.172 (0.026)	-0.128 (0.064)
After	0.190 (0.040)	0.249 (0.027)	0.060 (0.048)	0.160 (0.040)	0.190 (0.025)	0.030 (0.047)
Change in $R^2$	-0.164 (0.072)	-0.018 (0.042)	<b>0.146</b> (0.083)	-0.140 (0.071)	0.018 (0.036)	<b>0.158</b> (0.080)
Science	C	T	$\Delta$ (T-C)	C	T	$\Delta$ (T-C)
Before	0.363 (0.052)	0.215 (0.025)	-0.148 (0.058)	0.295 (0.055)	0.148 (0.022)	-0.147 (0.059)
After	0.173 (0.048)	0.210 (0.023)	0.037 (0.053)	0.128 (0.038)	0.142 (0.019)	0.013 (0.042)
Change in $R^2$	-0.190 (0.071)	-0.005 (0.034)	<b>0.185</b> (0.079)	-0.166 (0.066)	-0.006 (0.029)	<b>0.160</b> (0.073)

*Notes:* Table entries are  $R^2$  measures of **IEOp** (Equation (3.7)). Robust standard errors are in parentheses and were calculated using replication weights following the method as explained in Appendix III.1.5, clustering at the federal state level. **DiD** results are estimated according to Equation (3.9) taking into account population weights and the indicated fixed effects. Positive changes in  $R^2$  indicate increasing **IEOp** or decreasing **EEOp** and vice versa for negative changes.

*Background variables used to derive  $R^2$ :*

- (i) Individual Characteristics (IC) I: age and gender
- (ii) Individual Characteristics (IC) II: language spoken at home; migration background (based on parental birth place)
- (iii) Parental Characteristics (PC): highest parents' qualification (**ISCED**-level 1-2/**ISCED**-level 3-4/**ISCED**-level 5-6)
- (iv) Socio-economic Status (**SES**) I: no. of books in household (max. 11, 11-100, 101-500, more than 500)
- (v) Socio-economic Status (**SES**) II : highest **ISEI**-level-index[0-90] of job in the family
- (vi) Family Characteristics (FC) I: family structure - growing up in single parent household?
- (vii) Family Characteristics (FC) II: mother/father working part-time (PT) - mother/father unemployed (UE) - mother/father out of labor force (OLF)

*Compare:* Due to space constraints first-step regressions for T vs. C/C1/C2 have been omitted, but they are available upon request from the author.

*Source:* Author's calculations based on **PISA** 2003, 2006, 2009 and 2012.

**Table III.12:** Difference-in-Differences Results: Overview Control Group C

(1) Outcome	(2) Treatment	(3) Control	(4) Control set	(5) $R^2$ adj.	(6) $R^2$
reading	T	C	1	0.060	0.063
reading	T	C	2	0.073	0.078
reading	T	C	3	0.081	0.090
reading	T	C	4	0.086	0.095
reading	T	C	5	0.076	0.087
reading	T	C	6	0.096	0.113
reading	T1	C	1	0.058	0.062
reading	T1	C	2	0.072	0.078
reading	T1	C	3	0.080	0.089
reading	T1	C	4	0.085	0.095
reading	T1	C	5	0.075	0.086
reading	T1	C	6	0.095	0.112
reading	T2	C	1	0.036	0.041
reading	T2	C	2	0.044	0.051
reading	T2	C	3	0.051	0.062
reading	T2	C	4	0.056	0.067
reading	T2	C	5	0.046	0.059
reading	T2	C	6	0.067	0.087
mathematics	T	C	1	0.109	0.110
mathematics	T	C	2	0.121	0.124
mathematics	T	C	3	0.127	0.131
mathematics	T	C	4	0.134	0.139
mathematics	T	C	5	0.134	0.140
mathematics	T	C	6	0.136	0.146
mathematics	T1	C	1	0.101	0.102
mathematics	T1	C	2	0.114	0.117
mathematics	T1	C	3	0.120	0.125
mathematics	T1	C	4	0.128	0.133
mathematics	T1	C	5	0.127	0.133
mathematics	T1	C	6	0.129	0.139
mathematics	T2	C	1	0.097	0.099
mathematics	T2	C	2	0.106	0.110
mathematics	T2	C	3	0.109	0.115
mathematics	T2	C	4	0.117	0.123
mathematics	T2	C	5	0.116	0.123
mathematics	T2	C	6	0.120	0.132
science	T	C	1	0.153	0.153
science	T	C	2	0.156	0.158
science	T	C	3	0.160	0.164
science	T	C	4	0.169	0.172
science	T	C	5	0.162	0.166
science	T	C	6	0.177	0.185
science	T1	C	1	0.156	0.155
science	T1	C	2	0.159	0.160
science	T1	C	3	0.164	0.167
science	T1	C	4	0.173	0.176
science	T1	C	5	0.165	0.169
science	T1	C	6	0.182	0.189
science	T2	C	1	0.135	0.136
science	T2	C	2	0.135	0.137
science	T2	C	3	0.137	0.142
science	T2	C	4	0.144	0.149
science	T2	C	5	0.136	0.143
science	T2	C	6	0.155	0.164

*Notes:* This table shows  $T/T1/T2$  vs.  $C$  for all 3 test score domains with *school* fixed effects and for each version adding all 6 control sets from  $1 = [(i) + (ii)]$  until  $6 = [(i) + (ii) + (iii) + (iv) + (v) + (vi) + (vii)]$ . Note that columns (5), (7) show the DiD results using  $R^2$ , columns (6),(8) show the same but using adjusted  $R^2$  as IEOp measure.



**Table III.13:** Difference-in-Differences Results: Overview Control Group C-NT

(1) Outcome	(2) Treatment	(3) Control	(4) Control set	(5) $R^2$ adj.	(6) $R^2$
reading	T	C-NT	1	0.027	0.025
reading	T	C-NT	2	0.030	0.028
reading	T	C-NT	3	0.021	0.019
reading	T	C-NT	4	0.025	0.023
reading	T	C-NT	5	0.023	0.021
reading	T	C-NT	6	0.028	0.025
reading	T1	C-NT	1	0.025	0.023
reading	T1	C-NT	2	0.030	0.027
reading	T1	C-NT	3	0.021	0.018
reading	T1	C-NT	4	0.024	0.022
reading	T1	C-NT	5	0.022	0.019
reading	T1	C-NT	6	0.028	0.024
reading	T2	C-NT	1	0.003	0.002
reading	T2	C-NT	2	0.002	0.001
reading	T2	C-NT	3	-0.008	-0.010
reading	T2	C-NT	4	-0.006	-0.006
reading	T2	C-NT	5	-0.007	-0.007
reading	T2	C-NT	6	0.000	-0.002
mathematics	T	C-NT	1	-0.005	-0.006
mathematics	T	C-NT	2	-0.001	-0.003
mathematics	T	C-NT	3	-0.009	-0.010
mathematics	T	C-NT	4	0.000	-0.002
mathematics	T	C-NT	5	0.016	0.014
mathematics	T	C-NT	6	0.022	0.018
mathematics	T1	C-NT	1	-0.013	-0.014
mathematics	T1	C-NT	2	-0.008	-0.010
mathematics	T1	C-NT	3	-0.015	-0.017
mathematics	T1	C-NT	4	-0.006	-0.008
mathematics	T1	C-NT	5	0.009	0.007
mathematics	T1	C-NT	6	0.015	0.011
mathematics	T2	C-NT	1	-0.017	-0.017
mathematics	T2	C-NT	2	-0.016	-0.016
mathematics	T2	C-NT	3	-0.026	-0.027
mathematics	T2	C-NT	4	-0.018	-0.018
mathematics	T2	C-NT	5	-0.002	-0.003
mathematics	T2	C-NT	6	0.006	0.003
science	T	C-NT	1	0.040	0.038
science	T	C-NT	2	0.039	0.036
science	T	C-NT	3	0.026	0.023
science	T	C-NT	4	0.034	0.031
science	T	C-NT	5	0.038	0.034
science	T	C-NT	6	0.052	0.047
science	T1	C-NT	1	0.042	0.040
science	T1	C-NT	2	0.041	0.039
science	T1	C-NT	3	0.030	0.026
science	T1	C-NT	4	0.038	0.035
science	T1	C-NT	5	0.041	0.038
science	T1	C-NT	6	0.057	0.051
science	T2	C-NT	1	0.022	0.021
science	T2	C-NT	2	0.017	0.016
science	T2	C-NT	3	0.003	0.001
science	T2	C-NT	4	0.009	0.008
science	T2	C-NT	5	0.013	0.011
science	T2	C-NT	6	0.030	0.026

*Notes:* This table shows  $T/T1/T2$  vs.  $C-NT$  *Never-Takers* for all 3 test score domains with *school* fixed effects and for each version adding all 6 control sets from 1 = [(i) + (ii)] until 6 = [(i) + (ii) + (iii) + (iv) + (v) + (vi) + (vii)]. Note that columns (5), (7) show the **DiD** results using  $R^2$ , columns (6),(8) show the same but using adjusted  $R^2$  as **IEOp** measure.



### III.1.3 Further Details on the G-8 Reform

#### Related Literature on the Reform

Despite the public controversy over the **G-8 reform** that has even induced some federal states to reverse it (last column in **Table III.1** in Appendix **III.1.2**), few studies have evaluated the **G-8 reform** and its effects on outcomes such as educational achievement. To begin with, studies have analyzed the reform by comparing **G-8 model** and **G-9 model** cohorts within one federal state.

In most federal states the respective statistical offices have conducted studies comparing students' results in central exit examinations (*Abitur*) in the *double cohort*, that is the year when both the last **G-9 model** and the first **G-8 model** cohorts graduated from the *Gymnasium* (**Figure 3-1**). Generally, these statistical evaluations have found no systematic performance difference in central exit exams between students with eight or nine years of schooling. However, as grades in final exams are a useful performance indicator only within the same student cohort, comparisons over years based on marks have limitations. In fact, school exams are usually graded on the basis of a relative performance distribution in the respective year. Thus, using grades as outcomes may be of limited value for learning about the reform's impact on cognitive skills, in contrast to standardized test scores.

Furthermore, for the federal state of Saxony-Anhalt (ST), a small series of papers has analyzed different aspects of the **G-8 reform** (**Thiel et al. 2014**, **Büttner & Thomsen 2015**, **Meyer & Thomsen 2016**). In summary, they examine the reform's effects on academic achievement in central exit examinations 2007, when the *double cohort* graduated in ST (**Table III.1**). Findings show that - due to more intense schooling - exam results significantly deteriorated for mathematics, but remained unaffected for German literature. This suggests that **learning intensity** ratios differ across subjects. Moreover, no significant negative effects on students' soft skills are detected; opposing claims that increased **learning intensity** and accordingly reduced time for non-school related activities may have adversely affected non-cognitive skill formation. In line with this result, **Quis & Reif (2017)** show that the more intense schooling experience had only limited impact on students' health. However, due to reduced leisure time, **G-8 model** students were less able to relax and slightly more stressed compared to their peers in the **G-9 model**. Finally, **Meyer & Thomsen (2016)** find no negative effects of the **G-8 reform** on the ability, motivation and likelihood of students' entering university education.<sup>35</sup> Conducting a similar analysis for all German federal states, **Marcus & Zambre (2019)** show that the **G-8 reform** reduced enrollment rates at university and increased the likelihood of affected students to switch their major degree.

<sup>35</sup>But the reform influenced post-secondary school decisions. For instance, they find significant delays in the starting dates for a first university degree for female students who graduated from a **G-8 model** school. Instead, they were more likely to first complete a type of vocational education. Moreover, **Meyer & Thomsen (2016)** reveal that despite the **G-8 reform**, students continue to pursue their hobbies. However, they tend to work less outside of school.

Recently, a few papers have started to use more representative data that are more independent from school system related characteristics or relative performance measurement issues arising with marks at school (e.g. [PISA](#) data). Moreover, identifying the [G-8 reform](#) effect by exploiting the variation in its implementation across states and over time, this approach allows overcoming the shortcomings of previous studies. For instance, two papers related to this project exploit the reform setting using standardized [PISA](#) test scores for academic-track students as educational outcome variable.<sup>36</sup>

[Andrietti \(2016\)](#) uses this representative dataset in order to exploit the [G-8 reform](#) for conducting a Difference-in-Differences ([DiD](#)) estimation. He finds that the average treatment effect of the reform is significant and positive in all three educational outcomes (mathematics, reading and science). Treated students in a [G-8 model](#) experience an improvement of about 0.095 to 0.145 standard deviations in [PISA](#) test scores. In contrast to [Huebener & Marcus \(2017\)](#), [Andrietti \(2016\)](#) finds no evidence for an increase in general grade retention rates. Instead, his results suggest that grade repetition only slightly increased for boys and students with a migration background. This may indicate that the [G-8 reform](#) caused distributional changes in educational outcomes and thus may have affected [IEOp](#). However, [Andrietti \(2016\)](#) does not address distributional outcomes.

[Huebener et al. \(2017\)](#) use state regulations of timetables for secondary school to show that, due to the [G-8 reform](#), weekly instruction hours for the average treated student increased by about 6.5 percent over a period of five years. They suggest that increased instruction time improved the average student performance in all three [PISA](#) test domains. However, the effect size is small, with about six percentage points of a standard deviation in scores. Moreover, the effects are insignificant for low-performing students, whereas their high-performing peers experience significant, but small positive effects. This suggests that the performance gap among students in [Gymnasium](#) widened. In that regard, [Huebener et al. \(2017\)](#) focus on the increased instruction time effect, whereas [Andrietti \(2016\)](#) puts more emphasis on the increased [learning intensity](#) aspect of the reform.

In [Chapter 3](#), I use similar data as [Huebener et al. \(2017\)](#) with [PISA](#) test scores from 2000 to 2012. However, my focus is on analyzing the effects of increased [learning intensity](#) on educational outcomes in response to the [G-8 reform](#) (interpreting the reform similar to [Andrietti \(2016\)](#)). While these studies estimate the direct reform effect on test scores, they do not tackle the question whether increasing [learning intensity](#) may have changed Inequality of Educational Opportunity ([IEOp](#)). In [Chapter 3](#), I shift focus in the analysis of the [G-8 reform](#) onto distributional concerns, that is its consequences on [IEOp](#). In other words, I answer the question of whether the [G-8 reform](#) is *selective*, i.e. a reform that at least maintains test score results, but at the same time increases [IEOp](#); or whether it is *inclusive*, i.e. a reform that at least maintains test score results while decreasing [IEOp](#) ([Checchi & van de Werfhorst 2018](#)).

<sup>36</sup>Back in 2012, [Camarero Garcia \(2012\)](#) appears to have been the first to combine the usage of [PISA](#) test scores as an outcome variable to analyze the effects of the [G-8 reform](#) on cognitive skills in a Difference-in-Differences ([DiD](#)) estimation framework, finding a positive effect of about 0.15 standard deviations in test scores, with stronger effects for students with a migration background similar to the later results by [Andrietti \(2016\)](#).

Thus, I am among the first evaluating the **G-8 reform** based on Germany specific **PISA** data in order to analyze its impact on **IEOp**.

### The Reform Debate

The first **PISA**-study in 2000 received broad public attention in Germany, because it revealed that German students achieved weak test scores which were below the average of **OECD** countries (the so-called “**PISA**-shock”). Debates over how to improve the German school system ensued (e.g. Davoli & Entorf (2018)). Among the reform proposals, shortening the academic secondary school track (*Gymnasium*) from nine to eight years, the **G-8 reform**, remains controversial to this day. The last column in Appendix **Table III.1** gives an overview on the status quo of the reform as of school year 2015/16.

Mainly three reasons were given for introducing the **G-8 reform**. First, it was intended to reduce the relatively high age of university graduates in Germany. This was said to increase their competitiveness on the labor market compared to the (on average) younger graduates in other **OECD** countries (OECD 2005a). Furthermore, with students entering the job market one year earlier, working lifetime would be extended. Thus, the reform was said to contribute to stabilizing the social security system of a society facing demographic change. For instance, younger university graduates would start paying social security contributions earlier and over a longer time span. Second, as the most successful countries in the **PISA** test ranking, such as Finland, had a school system of twelve years, reduced schooling appeared to be both successful and efficient. Third, the **G-8 reform** was seen as a necessary adjustment of secondary school with regards to harmonizing tertiary education across Europe. As Büttner & Thomsen (2015) illustrate, the reform of shortening secondary school duration was also enacted in the context of the *Bologna Process*. This initiative aims to create a European Higher Education Area (EHEA) providing a more comparable, flexible European framework for tertiary education. Therefore, adjusting secondary school duration towards the average among other European nations was regarded to be sensible. Finally, one argument said that the reform would serve as an incentive for then younger school graduates to strive for obtaining a university degree; which would, thus, increase Germany’s below average rate of university graduates per birth cohort in comparison to other **OECD** countries.

However, opponents of the reform claimed that the intensified educational experience may worsen the human capital skill formation for affected students. Furthermore, parental complaints about increased stress for students (due to less free time) revealed further concerns. In fact, many parents said that compressed and intensified schooling may have negative impacts for their children, on both academic performance and the development of non-cognitive skills which are typically formed by non-academic recreational activities (Thiel et al. 2014). However, the majority of East Germans support shortened duration of the academic track, whereas the opposite is true across West German federal states that only recently adopted the **G-8 model** (Wössmann et al. 2015).

### III.1.4 Further Details on the Data used

#### Background Information on the PISA Data

Since 2000, the **OECD** conducts every three years the **PISA** study in order to measure the performance of 15 year-old students with respect to three basic competencies (*Life skills*), namely *reading*, *mathematical* and *scientific* literacy. These skills are regarded to be of special importance for a person's future success and are tested when students approach the end of compulsory schooling age (cf. **OECD (2010)**; **OECD (2013a)**). The idea of **PISA** is to evaluate the ability to apply knowledge, as acquired through the curriculum at school in the three tested domains, for solving real-world problems. This means to test the level of skills that students achieve until compulsory schooling ends and that are essential for participating in modern society (**OECD 2001**).<sup>37</sup> Apart from cognitive test scores, **PISA** collects rich information on family and school characteristics. This is based on questionnaires that students, their parents, teachers and school's principals fill out.

Concerning the **PISA** procedure, for each test cycle, the **OECD** chooses an international contractor who is responsible for the test's design and comparability: both across countries (e.g. that test questions are robust to cultural bias) and over time (making trend analysis possible (**OECD 2009b**)). On the country level, a **PISA** National Project Manager is chosen to make sure that the test is conducted according to the strict **OECD** quality guidelines. The test procedure itself resembles a *two-stage stratified randomized survey test design*. First, as a primary sampling unit, schools with eligible students are randomly selected (with a minimum of 150 schools in each country) to get a representative sample of all school types across all regions within a country. Then, as second-step sampling units, eligible students (15-year-olds)<sup>38</sup> are randomly selected within the sampled schools to reach a minimum of 4500 observations. Each student within a school receives distinct combinations of approved test questions on all three **PISA** domains.<sup>39</sup>

The level and scope of the test is identical for each student independent of the secondary school type attended. The paper-based test takes two hours, with additional 30 minutes dedicated for students to complete the questionnaire on their socio-economic background, school and on their attitude, motivation or aspiration. After the test has been evaluated on the national level (supervised by the international contractor), the **OECD** publishes a cross-country comparison of official test scores.

<sup>37</sup>The underlying question of **PISA** is "What is important for citizens to know and be able to do?". More generally, in **PISA** the concept of "literacy" refers to "students' capacity to apply knowledge and skills in key subjects, and to analyze, reason and communicate effectively as they identify, interpret and solve problems in a variety of situations". For specific definitions of each tested domain, I refer to **OECD (2004)** and in particular to chapter 1 of **OECD (2009b)**.

<sup>38</sup>This includes students who were aged between 15 years and 3 months and 16 years and 2 months at the beginning of the assessment period (plus/minus 1 month), who were enrolled in an educational institution (grade 7 or higher) (**OECD 2013b**).

<sup>39</sup>For details on the international **PISA** test procedure, I refer to section 2 in **Lavy (2015)** and to publications on the **PISA** Assessment Framework or to one of the Technical Reports on the test, e.g. **OECD (2013a)** and **OECD (2012)**.

To have comparable measures of latent ability in each **PISA** domain across and within countries, the raw answers to test questions, *items*, undergo some processing (cf. **OECD (2005b)**, **OECD (2009a)**, **OECD (2012)**). The so-called *Item Response theory (IRT)* is used to back out the distribution of the latent variable, cognitive skills (as measured by test scores), from individual *item* responses, taking into account the particular difficulty of an *item*. However, to address the issue of small-sample measurement error, for instance, because not all students answer all *items*, *Plausible Values* of test results are provided for each student.

First, the marginal distribution of the latent variable conditional on the *item* responses and a set of observables is estimated. Thus, for each student a probability distribution of test scores based on their answers is estimated. Second,  $M$  draws from this distribution are taken to become the *Plausible Values* of a student's test score. For **PISA**, in each test cycle, five *Plausible Values* are provided for each student in all three test domains ( $M = 5$ ). Conducting estimations with **PISA** test scores, the **OECD (2010)** suggests estimating any statistic  $s$  by using each of  $M$  *Plausible Values* datasets separately (getting  $\hat{s}_m$ ) and then averaging them over  $M$  to get a final estimate  $\hat{s}$ .

After this IRT-adjustment, the plausible test scores are standardized, as follows:

$$y_{ij} = \hat{\mu} + \frac{\hat{\sigma}}{\sigma}(x_{ij} - \mu) \quad (11)$$

where,  $x_{ij}$  is the post-IRT, pre-standardized score for student  $i$ , in country  $j$ ;  $\mu$  ( $\sigma$ ) are original mean (standard deviation) across all countries in the sample of the respective test year, and  $\hat{\mu}$  ( $\hat{\sigma}$ ) denote the estimated mean (standard deviation) for a country-specific sample based on the *Plausible Values*. This generates the normalized distribution of test scores with a mean value of 500 and a standard deviation of 100 test score points.<sup>40</sup>

The **PISA** test scores have neither maximum nor minimum values and there are no thresholds for passing the test, as it is designed to provide a relative measure that allows us to compare skills in the three domains across students and over time. The interpretation of test scores is eased when one compares them to a standard, such as *proficiency levels*. For instance, in mathematics, a proficiency level is supposed to consist of about 70 points. This corresponds to about two years of schooling in the average **OECD** country (**OECD 2013b**).<sup>41</sup>

<sup>40</sup>This means that across all **OECD** countries, the typical student scored 500 points in mathematics and about two-thirds of students in **OECD** countries between 400 and 600 points. Thus, 100 points constitute a huge difference in skills. To deal with difficulties in constructing meaningful measures of **IEOp** based on these standardized test scores, the variance is a useful index as explained by **Ferreira & Gignoux (2013)**.

<sup>41</sup>For instance, in *PISA-I-2012* the average difference in mathematics test scores between top and bottom quarters of students in **OECD** countries is 128 score points. However, most differences related to socio-demographic characteristics are smaller than an entire *proficiency level*. For example, across all **OECD** members in *PISA-I-2012*, on average boys outscore girls in mathematics by 11 points and native students score about 34 points higher than their peers with a migration background. Socio-economically advantaged students (in the top quarter of **SES**) score an average of 90 points higher than their disadvantaged peers (bottom quarter) (see Table II.2.4a in **OECD (2013b)**).



In contrast to GPA or final exam marks in school, which are only valid relative measures of performance in the respective school, **PISA** test scores have the important advantage to be a representative measure of cognitive skills for tested student cohorts across schools. Thus, **PISA** test scores make it possible to compare student cohorts both over time and across or within countries (federal states). However, three doubts on the validity of **PISA** test scores should be considered. First, if the student population from which the test participants are selected is not complete, as some students are excluded, this would threaten representativeness. However, the sampling standards of **PISA** require that participating countries cannot exclude more than 5% of students from the eligible population. Permissible reasons include only special cases, such as serious illnesses or lack of language skills due to recent immigration (e.g. asylum seekers). For Germany, with at least 97% of students in the eligible age (or in the ninth grade, see [Section 3.3.1](#)) being part of the initial student population, exclusion is not a concern for the validity of **PISA** data ([OECD 2010](#)); ([OECD 2013a](#)).

Second, one may be concerned that the actual participation rate of randomly selected students may be low, such that systematic selection may affect representativeness. However, for most developed countries the rate of compliers is above 80% for selected students and 85% for selected schools, surpassing **OECD** quality thresholds for the sampling process. In Germany, the participation rate of selected students is well above 80% (on average 92%), for schools, it has usually been even 100%. Moreover, there is no evidence for selection on observables for those selected who do actually not take the test ([Klieme et al. 2011](#)).

Third, another concern is that schools or more specifically teachers may bias comparability of scores, if they systematically train or motivate students for the test. However, based on student information about their motivation for the test and based on the information about how teachers prepared students for the test, as provided in the questionnaires of **PISA** test studies 2000-2012, such concerns are unwarranted ([Klieme et al. 2011](#)). The majority of teachers report that they tried to make students familiar with general testing strategies, but did not train them specifically for the test. In fact, affected students and teachers are only informed about their participation in the **PISA** test around two months before the test takes place. Moreover, given the general low probability of being selected for the test and as there are no incentives for neither teachers nor students to prepare for it, potential preparation could have only very limited effects on results.<sup>42</sup> Moreover, [Klieme et al. \(2011\)](#) show that the correlation between test motivation and scores is zero (on average 0.05) and did not change as more tested students were taught in the **G-8 model**. Thus, test results in Germany are not systematically influenced by any preparation behaviour or test motivation ([Wössmann 2010](#)).

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<sup>42</sup>Only half of the teachers indicated that they had talked with their students about **PISA** and those who did started not earlier than one month before the test. Vice versa, only 25% of participating students indicate to have prepared for the reading part, only 13% for mathematics, and only 8% for the science section in the test.

In conclusion, the advantages of using **PISA** data as measure of cognitive skills dominate any potential caveats, which is the reason why I decided to use them - in line with the studies mentioned in Appendix III.1.3. For the purpose of analyzing the effect of increased learning intensity (due to the **G-8 reform**) on **IEOp**, I use the Germany-specific versions of the **PISA** as explained in Section 3.3.1.

### Data Sources

For more information on the Germany-specific **PISA** data of each test cycle and the availability of these datasets, the reader is recommended to refer to the Institut zur Qualitätsentwicklung im Bildungswesen (Institute for Educational Quality Improvement) (IQB).

- PISA-2000:  
Artelt, C., Klieme, E., Neubrand, M., Prenzel, M., Schiefele, U., Schneider, W., Tillmann, K.-J., & Weiß, M. (2009). *Program for International Student Assessment 2000 (PISA 2000)*. Version: 1. IQB – Institut zur Qualitätsentwicklung im Bildungswesen. Datensatz [Dataset]. [http://doi.org/10.5159/IQB\\_PISA\\_2000\\_v1](http://doi.org/10.5159/IQB_PISA_2000_v1)
- PISA-2003:  
Prenzel, M., Baumert, J., Blum, W., Lehmann, R., Leutner, D., Neubrand, M., Pekrun, R., Rolff, H.-G., Rost, J., & Schiefele, U. (2007): *Program for International Student Assessment 2003 (PISA 2003)*. Version: 1. IQB – Institut zur Qualitätsentwicklung im Bildungswesen. Datensatz [Dataset]. [http://doi.org/10.5159/IQB\\_PISA\\_2003\\_v1](http://doi.org/10.5159/IQB_PISA_2003_v1)
- PISA-2006:  
Artelt, C., Baumert, J., Blum, W., Hammann, M., Klieme, E., & Pekrun, R. (2010): *Program for International Student Assessment 2006 (PISA 2006)*. Version: 1. IQB – Institut zur Qualitätsentwicklung im Bildungswesen. Datensatz [Dataset]. [http://doi.org/10.5159/IQB\\_PISA\\_2006\\_v1](http://doi.org/10.5159/IQB_PISA_2006_v1)
- PISA-2009:  
Artelt, C., Hartig, J., Jude, N., Köller, O., Prenzel, M., Schneider, W., & Stanat, P. (2013): *Program for International Student Assessment 2009 (PISA 2009)*. Version: 1. IQB – Institut zur Qualitätsentwicklung im Bildungswesen. Datensatz [Dataset]. [http://doi.org/10.5159/IQB\\_PISA\\_2009\\_v1](http://doi.org/10.5159/IQB_PISA_2009_v1)
- PISA-2012:  
Sälzer, C., Klieme, E., Köller, O., Mang, J., Heine, J.-H., Schiepe-Tiska, A., & Müller, K. (2015): *Program for International Student Assessment 2012 (PISA 2012)*. Version: 2. IQB – Institut zur Qualitätsentwicklung im Bildungswesen. Datensatz [Dataset]. [http://doi.org/10.5159/IQB\\_PISA\\_2012\\_v2](http://doi.org/10.5159/IQB_PISA_2012_v2)

### III.1.5 Empirical Strategy and Robustness

#### On the Computation of Standard Errors including replication weights

Throughout the paper, for both steps of the Difference-in-Differences (DiD) regressions (Section 3.4), the standard errors are computed in a way to take into account that student performance is reported in **Plausible Values** (PVs) of **PISA** test scores. Although, taking the average of five PVs as a measure of individual performance guarantees that estimates of group level means and regression coefficients remain unbiased, measures of dispersion should consider the within-student variability in PVs.

As explained by the **OECD (2009b)**, standard errors are computed by regressing five times on the dependent variable, individual test scores, thereby using all **Plausible Values** (PVs) in turn. For each regression, the sampling variance (*SV*) estimate is clustered at the federal state level. The final *SV* is given by the average of sampling variances obtained with the five PVs. In addition, standard errors are inflated by the imputation variance (*IV*), because test scores measure latent cognitive skills with error. The *IV* is estimated as the average squared deviation between the estimates obtained with each **Plausible Value** and the final estimate (using the average of PVs), with the appropriate degree of freedom adjustment ( $IV = \frac{1}{4} \sum (\hat{\theta}_i - \hat{\theta})^2$  where  $\hat{\theta}_i$  is the estimate for each of the five PVs and  $\hat{\theta}$  is the final estimate). Then, as shown by **OECD (2009b)**, the final error variance *TV* can be obtained by combining the sampling and imputation variance as follows:

$$TV = SV + \left(1 + \frac{1}{K}\right) * IV = SV + 1.2 * IV \quad (12)$$

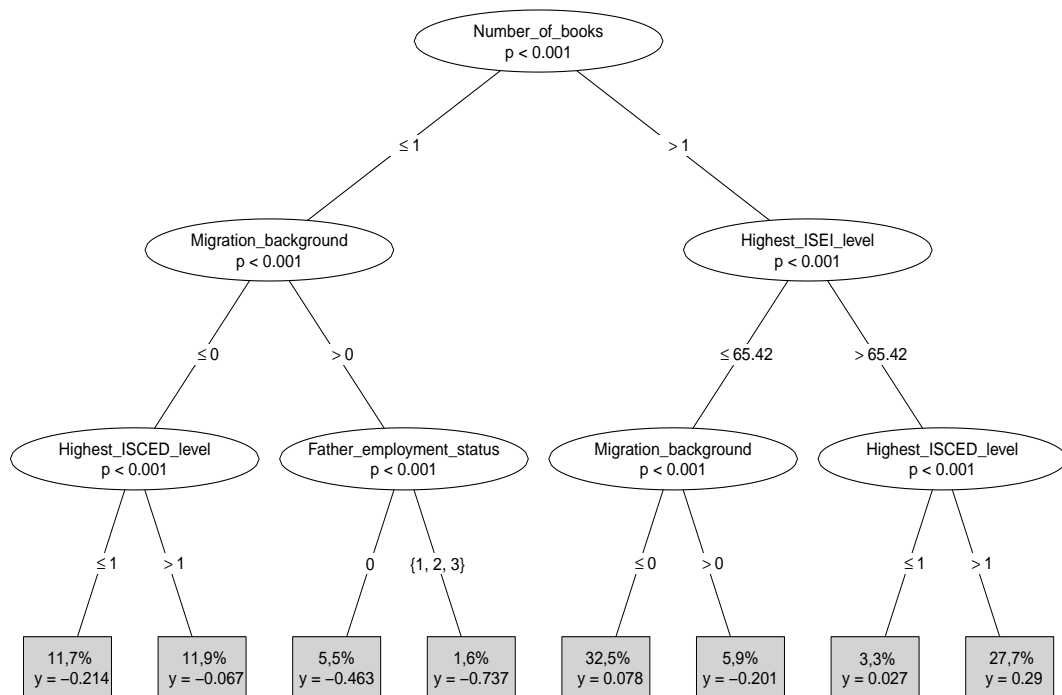
where  $K = 5$  is the number of **Plausible Values** for each student. The final standard errors are given by the squared roots of the final error variances. To estimate *SV*, one can apply Fay's variant of the *Balanced Repeated Replication (BRR) method*, which directly takes into account the two-stage stratified sampling design of the **PISA** test. For this method, each regression is iterated over the 80 sets of *replication weights* provided in the **PISA** dataset. Then, the *SV* estimate is given by the average squared deviation between the replicated estimates and the estimate obtained with final weights, with a degree of freedom correction depending on the Fay coefficient (a parameter that governs the variability between different sets of replication weights, set at 0.5 in the **PISA** study).

Standard errors in all *first-step* and *second-step* regressions are based on this method. For computational convenience and similar to **Philippis & Rossi (2019)**, I use the “un-biased shortcut” procedure described in **OECD (2009b)**. It relies on only one set of **Plausible Values** (PVs) for estimating the sampling variance (whereas the imputation is estimated using all five sets). **Andrietti (2016)** relies on clustering standard errors on the state level and argues that a wild t-bootstrap procedure produces similar results. **Huebener et al. (2017)** also focus on clustering methods. However, given the sampling strategy used to generate **PISA** scores, estimating standard errors considering both replication weights and PVs is more reliable.



### Detecting Important Circumstances Variables with Machine Learning

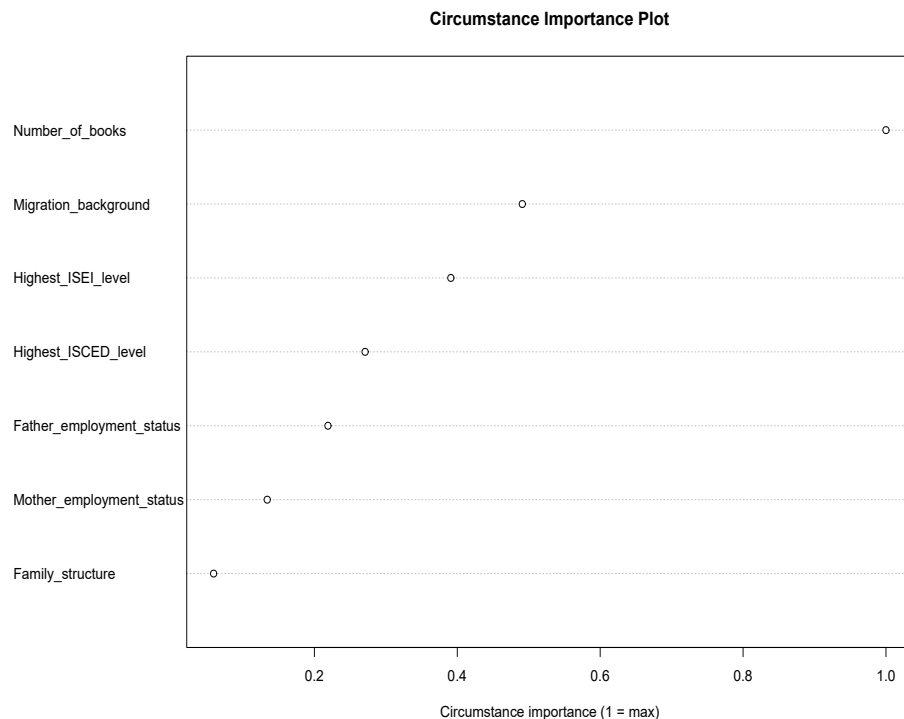
Machine Learning (ML) can be helpful to identify a model specification, based on its advantages of being a data-driven, transparent, theory-agnostic, non-parametric approach. I apply the ML method of conditional inference regression trees, in order to test the importance of my chosen *circumstances* in [Section 3.3.3](#). This exercise confirms that the selected *circumstances* are indeed relevant for explaining differences in cognitive skills as measured by [PISA](#) tests. My ML algorithm follows the approach of [Brunori et al. \(2019\)](#) and I refer to their paper for more details on the technicalities. In summary, the tree algorithm splits the dataset into groups if the null hypothesis of Equality of Educational Opportunity (EEOp) is rejected. This is best illustrated in the figure at the bottom of this page: it depicts an opportunity tree which is calculated for the standardized [PISA](#) mathematics score as outcome variable and is based on the [PISA-I](#) dataset. The tree shows that, for instance, students living in households that own less than 100 books achieve significantly worse results in mathematics compared to those from households with more books. Generally, the tree reveals that there are groups of certain *circumstances* along the lines of socioeconomic status, parental education, and migration background.



*Note:* This is an **Opportunity Tree** for students in *Gymnasium* considering [PISA-I](#) waves 2003, 2006, 2009 and 2012: Variables inside the white circles depict the *circumstances* on which the algorithm has chosen to split on. The splitting criterion value is shown on the tree-branches and is based on the p-value (at the one percent level) of the difference in test scores between the *circumstances* groups. Terminal nodes are depicted by grey boxes: The first number shows the respective group's percentage share of the total weighted sample size, the second number shows the group's predicted standardized mathematics score. The tree algorithm splits the dataset into groups, if the null hypothesis of [EEOp](#) is rejected. For illustrative reasons, the three depicted considers a maximum depth of 3. To more clearly identify all drivers of [IEOp](#), gender, as main driver of differences in test scores, was kept out.

To check if the results obtained by the tree are stable, I further conduct a conditional inference regression forest machine learning procedure. The method is similar to the regression tree, however, forests calculate many trees and then average the obtained effect over the identified subgroups. Therefore, only a variable importance plot can be depicted (see the figure above). The importance is calculated by the permutation principle of the mean decrease in accuracy whereby the variable importance is adjusted to depict relative terms. Hereby, the *circumstance* variable with greatest importance equals 1. Like in the tree algorithm, the number of books seems to be an important factor influencing PISA test scores of students in Germany. Moreover, migration background, the highest ISEI and ISCED level of the household in which a student grows up, turn out to be relevant *circumstances*. Thus, the machine learning algorithm confirms to include the depicted variables as controls, which is in line with the arguments provided in Section 3.3.3.

Furthermore, one can use the importance of different *circumstances* (as revealed by the plot graph) to derive refined interaction terms in order to detect heterogeneity in the causal reform effect on IEOp. In that regard, for instance, the regression forest exercise indicates that both the number of books in a household and the highest parental jobs' ISEI level should be used as important *circumstances* variables to control for social status.



*Note:* This figure depicts the **Circumstance Importance Plot** for students considering PISA-I waves 2003, 2006, 2009 and 2012: The plot indicates the importance of the listed *circumstances* with respect to the standardized PISA mathematics scores of students in *Gymnasium*. The importance is calculated by the permutation principle of the mean decrease in accuracy whereby the variable importance is adjusted to depict relative terms. Note that qualitatively similar results can be obtained using PISA test scores in reading or science as outcome variable, instead of only mathematics scores. Due to space constraints, the respective graphs for reading and science are only available upon request from the author.

Results in [Section 3.5.4](#) show that these *circumstances* explain heterogeneity in the effect of higher [learning intensity](#) on test scores, which is in line with the estimated increase in [IEOp](#).

#### List of *Circumstances Variables*

##### 1. Individual Characteristics (IC):

- (I) *gender* [Base: male] and *age* (in years)
- (II) *migration background* [Base: German] and *language spoken at home* [Base: German]

##### 2. Parental Characteristics (PC)

- (III) *education*: highest *ISCED*-index level in 3 categories [Base: ISCED-level (3-4)]

##### 3. Socio-Economic Status ([SES](#))

- (IV) *number of books in household* [Base: 101-500]
- (V) *highest ISEI-index level* [scale: 0-90]

##### 4. Family Characteristics (FC)

- (VI) *single parent household* [Base: none]
- (VII) *mother/father employment status* [Base: FT]

## Overview of Definitions and T/C-Groups

1. Concerning the time periods possible, one can define the following models:
  - *Baseline Model: medium-term perspective* (Base): covers time period (2003-2012)
  - *Robustness Model: short-term perspective* (Robust): covers time period (2003-2009)
2. Concerning *Treatment and Control Groups*, the following groups can be formed (Table 3.2)
  - *Treatment Group* (T): Baden-Württemberg (**BW**), Bavaria (**BV**), Lower Saxony (**LS**), Bremen (**BR**), Hamburg (**HB**)
  - *Treatment Group* (T1): Baden-Württemberg (**BW**), Bavaria (**BV**), Lower Saxony (**LS**)
  - *Treatment Group* (T2): **BW, BV, LS, BR, HB, Berlin (BE), Brandenburg (BB)**
  - *Control Group* (C): Rhineland-Palatinate (**RP**), Schleswig-Holstein (**SH**)
  - for *Model Robust* short-term models *Control Group* (C1): **RP, SH, North Rhine-Westphalia (NRW)**
  - *hypothetical Control Group* (Ch): Saxony (**SN**), Thuringia (**TH**)
  - *Never-Takers Control Group* (C-NT): **RP, SH, SN, TH**
3. *Neither Treatment nor Control Group*:
  - Saarland (**SL**), Saxony-Anhalt (**ST**), Mecklenburg-West Pomerania (**MWP**), Hesse (**H**)

## Further Aspects on the Internal Validity of Empirical Strategy

There were no specific changes in the political parties forming the government of federal states that form my main treatment and control group settings in both the *Model Base* (2003-2012) or *Model Robust* (2003-2009). Moreover, by conducting a Difference-in-Differences estimation (DiD) and controlling for federal states, general differences in the political parties in charge of implementing the reform are taken into account. The fact that there have not been systematic changes in governments across treatment and control groups around the respective reform time is supportive evidence, that for the period considered, it is plausible to assume a comparability in the stability of each federal state's educational policies.

- Treatment Groups (T/T1)
  - **BW**: Conservatives (CDU) led the government for decades until 2011, followed by (2011-2016) a coalition government of the Green Party/Social-Democrats (SPD): the reform was implemented by the CDU and it is plausible to assume that, due to the time lag for new government policy to take effect, educational policy up until year 2012/2013 was made by the same party.
  - **BV**: Conservatives (CSU) led the government over the whole analysis period (2003-2012), thus, it is plausible to assume that school policy was conducted by the same party.
  - **LS**: Conservatives (CDU) led the government over the whole analysis period (2003-2012); afterwards/beforehand the government was led by the SPD. It is plausible to assume that for the whole analysis period, school policy was made by the same party.
  - **BR**: Social-Democrats (SPD) led the government over the analysis period (2003-2012), and thus, it is plausible to assume that school policy was made by the same party.
  - **HB**: Social-Democrats (SPD) led the government for decades (until 2001, since 2011). In between Conservatives governed and thus it is plausible to assume that for the analysis period (2003-2012), school policy was mainly conducted by the same party.
- Control Groups (C/C1)
  - **RP**: Social-Democrats (SPD) led the government over the analysis period (2003-2012), thus, it is plausible to assume that school policy was conducted by the same party.
  - **SH**: Social-Democrats (SPD) led the government for decades (1988-2005, 2012-2017). In between (2005-2012), the government was led by Conservatives, from 2010-2012 in a grand coalition with the SPD. School policy remained similar during the analysis period.
  - **NRW**: Social-Democrats (SPD) led the government for decades (until 2005, 2010-2017). They had already enacted the reform, when for five years the government changed to the Conservatives (CDU) who continued the implementation of the reform. School policy remained similar, in particular, when taking NRW as control for the period 2003-2009.

Thus, by focusing on the analysis period (2003-2012) that covers only the first affected cohorts, the main **DiD** assumptions appear to hold. However, as some federal states decided to reverse the reform in recent years, a similar evaluation may be less plausible for the time period after 2012. The reform has become a debated topic in most federal states since the early 2010s (cf. last column in **Table III.1**). But for the very first affected cohorts, there are no systematic changes in governments when comparing treatment and control group states over the time period (2003-2012).

### On Ability in the context of Measuring IEOp and within the DiD framework

Even though, one may have concerns about differences in ability (or talents) when it comes to measure IEOp, one should consider the following. First, the IEOp measurement framework takes any time-invariant features of cognitive skills into account, as part of the unobserved component of *circumstances*. Second, recent literature in the field of neuroscience suggests that in the spirit of the Human Capital Theory, cognitive skills appear to be malleable, in particular during early childhood, through epigenetic processes. This may explain why, for instance, [Boca et al. \(2017\)](#) find that attending childcare institutions can significantly improve children's cognitive skills, in particular those from disadvantaged SES. Thus, the IEOp measurement framework fully takes the role of ability into account, both as unobserved *circumstance* and *effort*. Consequently, it is a lower bound measure. Moreover, skills are defined as mixture of *circumstances* and *efforts*.

Concerning the Differences-in-Differences estimation approach (DiD), the only assumption that I make is that, in general, the distribution in cognitive abilities of students between 2003 and 2012 did not systematically change across German federal states. Given the fact that moving behavior between federal states is unlikely to have occurred ([Section 3.4](#)), this means we assume that cognitive skills did not suddenly change across states during the analyzed time period for any other reason than the reform treatment. Moreover, even if general systematic differences in ability across federal states existed, the DiD framework would control for any general level differences in ability.

Therefore, given the short time period and the controls enacted via the DiD framework, it is hard to find plausible reasons why there should have been any significant changes in cognitive abilities that differ among federal states and could bias results. In any case, these thoughts should be of less concern in this quasi-experimental setting than in the settings of other research papers that measure IEOp across countries. Moreover, as the reform only affects students from age 10 onward, and treatment merely involves more intense instruction, but not different contents, I claim that these concerns - which can neither be addressed by empirical methods nor available data (eg. there are no representative data on IQs in Germany) - are of second order importance and comparable to those in other studies estimating returns to schooling.

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